THE VALUE OF BRANDS VERSUS ATTRIBUTES: CUE COMPETITION AND BLOCKING IN CONSUMER LEARNING

By

STIJN M. J. VAN OSSELAER

A DISSERTATION PRESENTED TO THE GRADUATE SCHOOL OF THE UNIVERSITY OF FLORIDA IN PARTIAL FULFILLMENT OF THE REQUIREMENTS FOR THE DEGREE OF DOCTOR OF PHILOSOPHY

UNIVERSITY OF FLORIDA

1998

ACKNOWLEDGEMENTS

This dissertation has benefited, directly and indirectly, from interactions with many people. I would like to thank these persons for many discussions, comments, and classes, and for supporting and encouraging me in my research. First of all, this dissertation has benefited tremendously from many discussions with Joe Alba, my dissertation chair. My thoughts on the matter of consumer learning and brand equity have also benefited from discussions with Chris Janiszewski and the other members of the dissertation committee: Alan Cooke, Joffre Swait, Bart Weitz, and Joel Houston. The dissertation has further benefited from discussions with several students, former students, and faculty at the University of Florida, especially Luk Warlop, Tom Meyvis, John Pracejus, Steve Shugan, and Erica van Herpen, as well as with participants in interviews at the 1997 AMA conference in Chicago and in seminars at Tilburg University, Washington University, the University of Chicago, the University of Michigan, the University of Colorado, Pennsylvania State University, INSEAD, Harvard University, Duke University, and the Massachussetts Institute of Technology.

Less directly, this dissertation has benefited from my mentors and teachers throughout my school career, especially Paul De Boeck, S. Ratneshwar, Paul Webley, Joel Cohen, John Lynch, Alan Sawyer, Mrs. Eckhardt and the late Mrs. Kragten. Even less directly, but certainly not less importantly, this dissertation has benefited from the continued encouragement of my curiosity and ambition by my family and my parents in

particular. I would like to give special thanks to my parents for the monetary, the practical, and especially the emotional support they have given me. Finally, I would like to thank my wife Ineke for making my life so much fun, certainly a conditio sine qua non for producing anything meaningful.

TABLE OF CONTENTS

APPENDICES

Abstract of Dissertation Presented to the Graduate School of the University of Florida in Partial Fulfillment of the Requirements for the Degree of Doctor of Philosophy

THE VALUE OF BRANDS VERSUS ATTRIBUTES: CUE COMPETITION AND BLOCKING IN CONSUMER LEARNING

By

Stijn M. J. van Osselaer

August 1998

Chairman: Joseph W. Alba
Major Department: Marketing

When consumers make purchasing decisions, they have to evaluate the consumption outcomes or benefits afforded by the products they are considering. Because consumption outcomes usually cannot be assessed directly in the store, consumers learn to use search features as indicators of consumption outcomes. When consumers are not aware of the attribute(s) that actually determine those outcomes, consumers may use brand names as the primary indicators of consumption outcomes and therefore may show strong brand preferences. However, when consumers are aware of a product's critical attribute(s), one would expect consumers to rely primarily on attribute information, because intrinsic attributes should have a more direct causal relationship with consumption outcomes. Thus, to the extent that brand equity depends on brand-driven differences in predicted consumption outcomes, brand equity should decrease when consumers are provided with predictive attribute information.

Seven experiments provided support for this prediction when consumers were exposed to brand and attribute information simultaneously. However, this effect did not occur or was strongly attenuated when consumers had just a few experiences with brand names before being exposed to predictive attribute information. Whereas consumers had little difficulty learning the predictive value of attributes when they were exposed to brand and attribute information simultaneously, consumers failed to learn the predictive value of attributes when they were pre-exposed to brand information. Thus, brand pre-exposure may "block" the acquisition of value by attributes, allowing brands to maintain equity in the face of predictive attribute information.

The experiments showed the blocking effect to be robust, e.g., to prior beliefs favoring attributes, a very low number of brand pre-exposures, and the absence of trial-by-trial prediction. In addition, significant blocking effects were found when positioning attributes accompanied brand names and when consumers experienced several brands that shared the same attributes. Results suggest that blocking is caused by an associative learning process instead of normative causal reasoning, truncated attention, or the use of principles of explanatory coherence. The experiments also suggest that, in general, brands and attributes compete for equity. Implications are discussed for theories of how brands and attributes acquire and lose customer-based equity.

CHAPTER 1
INTRODUCTION

During the past decade, brand equity has been the object of a substantial amount of research. Aside from discussions of the exact meaning of brand (e.g., Aaker 1991; Barwise 1993; Farquhar 1989; Keller 1993), most brand equity research has focused on three major issues. First, a number of researchers have investigated ways to measure brand equity, either at the firm level (e.g., Simon and Sullivan 1993) or at the level of the individual consumer (e.g., Kamakura and Russell 1993; Park and Srinivasan 1994; Srinivasan 1979; Swait, Erdem, Louviere, and Dubelaar 1993). The second issue concerns the transfer of brand equity across products (e.g., Aaker and Keller 1990; Broniarczyk and Alba 1994a; Erdem 1997; Keller and Aaker 1992; Montgomery and Wernerfelt 1992; Park, Milberg, and Lawson 1991; Sappington and Wernerfelt, 1985; Sullivan 1990, 1992; Wernerfelt 1988). The third issue, which has drawn much less direct attention, pertains to the psychological sources of brand equity. The question of interest here is why brands add value to a product in the eyes of the consumer. Several psychological mechanisms have been identified that can account for brands' acquisition of perceived value above the utility provided by the physical product per se, even in extreme cases in which branded products are compared to competing products that are identical in terms of their critical attributes.[1] For example, a certain branded product can

[1] "Critical attributes" are attributes that have a direct and important influence on product performance. The product classes used in the experiments are unfamiliar to the subjects, allowing me to arbitrarily refer to

be preferred over an otherwise identical competing product because the brand name, through its network of brand associations (Keller 1993), actually produces extra benefits if the brand name allows consumers to acquire or enhance a desired identity or sense of self (e.g., Belk 1988; Kleine, Kleine, and Kernan 1993; Richins 1994) or to manage specific impressions (e.g., Schlenker 1980). Thus, the brand can directly provide benefits beyond the physical performance of the product. Another mechanism that can lead consumers to evaluate identical products differently occurs when consumers' product experiences are ambiguous, making their perceptions of benefit levels (instead of actual benefit levels) dependent on brand associations formed from external sources such as advertising (e.g., Hoch and Ha 1986). A third mechanism that may lead consumers to place value on brand names at the expense of critical attributes is a lack of awareness of those critical attributes. For example, it is possible that many consumers are plainly unaware that both Tylenol and its generic competitors contain acetaminophen or that Head & Shoulders shares pyrithione zinc and all its other ingredients with a number of competitors. Several authors have demonstrated that search for information can be extremely limited (e.g., Dickson and Sawyer 1990; Hoyer 1984, Leong 1993). Consumers who are unaware of products' attributes may resort to the brand name as their only indicator of product quality, leading them to place a large amount of value on brand names.

The main question I address in this dissertation is what happens to brand equity and "attribute equity" when the sources of brand equity described above are ruled out by actually providing consumers with information about critical attributes. Does providing

---

certain intrinsic attributes that are perfectly correlated with product performance as "critical attributes."

consumers with critical attribute information necessarily lead them to place more value on critical attributes at the expense of the brand? In other words, does providing consumers with critical attribute information necessarily lead to reduced brand equity and increased competitive pressure from competitors who share the same critical attributes?

Beliefs About Brands and Attributes

One would expect that the answer to the questions stated above should be affirmative. A large body of research on covariation assessment, classification learning, and causal induction suggests that consumers should be heavily influenced by their prior beliefs in detecting feature-quality covariations and assigning value to product features (e.g., Alloy and Tabachnik 1984; Broniarczyk and Alba 1994b; Chapman and Chapman 1969; Cheng 1997; Einhorn and Hogarth 1986; Nisbett and Ross 1980; Sanbonmatsu, Akimoto, and Gibson 1994; Waldmann 1996; Waldmann and Holyoak 1992; White 1995). One would expect consumers to have fairly general prior beliefs about the potential causality of intrinsic attributes (e.g., acetaminophen) for consumption outcomes (e.g., headache relief) that would be favorable to the detection of the causal role of critical attributes for consumption outcomes. Indeed, consumers probably know that brand names, in contrast to intrinsic attributes, do not cause consumption outcomes. Thus, one would expect that if consumers are provided with statistically equally predictive information about brand names and critical attributes, they would judge the attribute information to be more causal than the brand information. The brand would be more likely to be seen as a spurious, and therefore unreliable, predictor of consumption outcomes, leading to a strong decrease in the brand's impact on product evaluations.

The term "critical attribute" was chosen purely for expositional reasons.

Blocking

However, there is reason to question this assumption. A classic finding in the conditioning literature suggests that brands may keep their value, at least in some contexts. In particular, consider the "blocking" paradigm reported by Kamin (1969). In an initial learning phase, Kamin repeatedly presented rats with a simple conditioned stimulus consisting of element A (a noise) paired with an unconditioned stimulus (an electric shock, schematically represented as "+"). After a number of trials, an association was formed between the noise (A) and the shock (+), and the rats showed a conditioned response to the noise (A) in anticipation of the shock. In a second learning phase, he paired a compound conditioned stimulus consisting of element A (noise) plus element B (a bright light) with the same type of shock (+), which also produced a conditioned response. In a third, test phase, element B (light) was presented by itself; in this case no response was found. Thus, when an animal is first conditioned to respond to a simple conditioned stimulus consisting of element A (noise) and is subsequently conditioned to respond to a compound conditioned stimulus consisting of element A (noise) plus element B (light), the second element (light) will not acquire any "predictive value." Responding to the second element (light) is "blocked" by pre-exposure to the first element (noise) despite the fact that both elements A and B are perfectly predictive of the unconditioned stimulus. Analogous effects have been observed in human predictive and causal learning contexts (e.g., Chapman 1991; Chapman and Robbins 1990; Dickinson, Shanks, and Evenden 1984; Shanks 1985; Waldmann and Holyoak 1992).

This phenomenon may be extended to consumer learning contexts. Like the rats in Kamin's experiment, consumers are presumably motivated to predict levels of

consumption outcomes (e.g., overall quality). For many products, consumption outcomes cannot be assessed directly in the store. In these situations, consumers will learn to use search features (e.g., brand names, ingredient information) as indicators of consumption outcomes. Thus, like the rats, consumers may be expected to use cues to predict relevant outcomes. To the extent that brand equity is defined as the difference in product evaluations due to a brand (cf. Farquhar 1989; Keller 1993), brand equity should be directly related to the extent to which the brand is regarded as a reliable predictor of product performance. That is, brand equity should depend on the predictive value of the brand or attribute in the eyes of consumers (e.g., Erdem and Swait 1998). Thus, a failure by a product feature to acquire predictive value (e.g., due to blocking by another feature) should decrease that feature's equity.

The blocking paradigm maps onto consumer learning when consumers, through their own experience or vicariously through advertising or word-of-mouth, first learn that consuming a brand (e.g., Tylenol) leads to a certain consumption outcome (e.g., fast headache relief) before they receive information about the critical ingredient(s) of that product (e.g., by looking at ingredient information).

Overview

In the remaining chapters of this dissertation I will first describe three process accounts of the blocking effect. Second, I will introduce a number of potential moderators and boundary conditions that are of importance in consumer learning contexts and that could limit the generalizability of the blocking effect. Third, I will report seven experiments that were designed (i) to determine whether brands can block attributes, (ii) to test potential moderators and find boundary conditions of the blocking phenomenon in

consumer learning contexts, and (iii) to explore the process underlying the blocking phenomenon. Finally, I will summarize the findings of the experiments and discuss their implications for our understanding of how brands acquire equity and, more generally, of how consumers learn to value products and their features.

CHAPTER 2
THEORETICAL BACKGROUND

Since the discovery of the blocking effect by Kamin, three major classes of explanations have surfaced that can explain why learning of the relation between one cue (A) and an outcome (+) can prevent the later acquisition of a response by a second cue (B), when it is presented in compound with the first cue (AB+).

Encoding Account

One of the first, but least supported, explanations of the blocking effect and some other effects in the animal and human literatures on predictive and causal learning is the "encoding account" (e.g., Price and Yates 1995). According to this account, subjects learn quickly which cue is most predictive of an outcome and then direct most of their attention to that cue. In a blocking situation, this account holds that learning about the first cue prevents subjects from attending to the second cue. Accordingly, subjects in the first phase of a blocking design learn that the first cue (A) perfectly predicts the outcome (+). Once they have learned that A perfectly predicts the outcome, they stop paying attention to other stimuli. As a result, subjects do not encode the occurrences of cue B, cannot reproduce the pattern of co-occurrence between cue B and the other cues or outcome, and do not learn that cue B is predictive of the outcome. This explanation will be discussed in more detail in the introduction to Experiment 4, which was specifically designed to test the encoding account.

Contingency Account

In the broader area of predictive, contingency, and causal learning, two groups of theories have dominated the debate over the past few years (see, e.g., Allan 1993, and Shanks, Medin, and Holyoak 1996 for an overview). One group of theories is commonly referred to as "contingency" models (e.g., Wasserman and Miller 1997), "rule-based" models (e.g., Allan 1993), or "normative" models (e.g., Baker, Murphy, and Vallée-Tourangeau 1996). The other group of theories is referred to as "associative" (e.g., Wasserman and Miller 1997) or "connectionist" models (e.g., Kruschke 1996).

According to contingency models, the modern offspring of Kelley's attribution theory (see, e.g., Kelley 1973), humans judge causal strength in two stages (e.g., Baker et al. 1996; Price and Yates 1995). First, during learning, they store information about their experiences in episodic memory (e.g., Baker et al. 1996; Price and Yates 1995). In a second stage, they retrospectively access this information, code it, try to compute the frequencies for all the available combinations of cues and outcomes, and use these frequencies (together with other abstract and specific causal knowledge) as input into normative causal models (e.g., Cheng 1997; Cheng and Holyoak 1995; Cheng and Novick 1992; Cheng, Park, Yarlas, and Holyoak 1996; Holyoak, Koh, and Nisbett 1989; Melz, Cheng, Holyoak, and Waldmann 1993; Spellman 1996a, 1996b; Waldmann 1996; Waldmann and Holyoak 1992). Thus, learners reason as if they were "intuitive scientists" when inducing causality (Cheng and Holyoak 1995; Spellman 1996a). In fact, they often behave as if they were conducting an analysis of variance. When there is only one potential cause, the perceived strength of the causal relationship between a potential cause and an outcome closely follows the normative ΔP contingency rule. According to

this rule, the causal strength between a potential cause (C) and an effect (E) is a function of ΔP, which is computed as

$$\Delta P = P(\mathrm{E}|\mathrm{C}) - P(\mathrm{E}|{\sim}\mathrm{C})$$

or the probability of the effect (E) given the presence of the potential cause (C) minus the probability of the effect in the absence of the potential cause. Note that the computation of ΔP requires an inference based on causal knowledge about the situation in which the potential cause is absent, i.e., that such cases represent "information of absence" instead of "absence of information." When there are multiple potential causes, the simple ΔP rule, or "simple contrast" is no longer normative because the target potential cause may be confounded with the truly causal factor (e.g., Cheng and Holyoak 1995, Spellman 1996a, 1996b). According to the contingency view, humans in such situations will seek to compute one or more "conditional contrasts" that are logically informative about the unconfounded influence of the target cue. Much like scientists, they will try to compute ΔP for a subsets of cases, a specific "focal set," in which all other potential causes are absent or in which the other causes are kept constant or vary randomly (Cheng and Holyoak 1995, Cheng 1997) and base their judgments of causal strength solely (Cheng et al. 1996, Cheng 1997) or partially (Cheng and Holyoak 1995) on the relevant independent contrast(s). When data are missing for one of the "cells" of the contingency table, subjects are expected to be uncertain about the causal strength of the cue for which the relevant conditional contrast(s) cannot be computed (Cheng 1997). According to Waldmann and Holyoak (1992), the same causal reasoning processes occur when subjects are asked to judge a cue's predictiveness for an outcome as when they make judgments about a cue's causal strength.

	B	~B
A	+	+
~A	?	-

FIGURE 1
FOUR-CELL REPRESENTATION OF BLOCKING PARADIGM

Contingency theories hold that subjects in a standard two-phase blocking design register all events and then try to compute the frequencies of the presence of the outcome for each combination of the two cues. The first phase provides information about the frequencies of two cue combinations--cue A present-cue B absent and cue A absent-cue B absent. In the former case, the outcome always occurs. In the latter case, the outcome never occurs. These two events can schematically be represented as follows: A~B+ and ~A~B-. The second phase also provides information for two cue combinations--cue A present-cue B absent and cue A absent-cue B absent. The first combination is always followed by the outcome. The second is never followed by the outcome. The schematic representation of these two events is AB+ and ~A~B-. Figure 1 puts the schematic representations together in a four-cell table. It can be seen from Figure 1 that information is missing for the ~AB cell in the blocking design. In the specific case of the blocking design (e.g., Waldmann and Holyoak 1992; Cheng 1997; Cheng et al. 1996), the logically informative conditional contrast to determine the causal strength of cue A is the contrast of A in the absence of B, and the critical conditional contrast for cue B is the contrast of B in the absence of A. The informative conditional contrast for cue A can be computed and is equal to 1. Hence, A will be judged to be a strong predictor of the outcome. However, the conditional contrast of B in the absence of A cannot be computed, because

information for the B present-A absent cell is missing. As a result, subjects are uncertain about the predictive strength of B. The introduction to Experiment 5 addresses this explanation in detail.

Associative Account

The associative or connectionist theories of how people learn to predict outcomes and judge the predictiveness, contingency, and causal strength of cues are inspired by research on classical conditioning in animals (e.g., Rescorla and Wagner 1972) and by research in neurophysiology (see, e.g., Rumelhart and McClelland 1986; McClelland and Rumelhart 1986) and engineering (e.g., Widrow and Hoff 1960). According to the associative or connectionist view (see, e.g., Allan 1993; Shanks 1994; Shanks and Dickinson 1987; Shanks, Medin, and Holyoak 1996; Young 1995), outcome predictions and judgments of predictiveness, contingency, and causal strength depend directly on the strengths of associations or connections that people form between cues and outcomes. In general, associations are strengthened every time a cue is followed by an outcome and weakened when the cue is not followed by the outcome. Thus, old and new information is represented in the associations among cues and outcomes. The associative system keeps no record of specific experiences. In other words, associative systems are predictive, forward-looking systems that have no episodic memory and are not capable of retrospective processing. When a cue is not presented, no updating takes place (see Van Hamme and Wasserman 1994 for an exception). The updating process is described by a simple learning rule that is usually error-driven; that is, the amount of updating taking place as a result of one experience depends on the difference between the expected

outcome and the actual outcome. When the outcome is perfectly predicted, the system keeps paying attention, but no updating takes place.

The blocking effect can easily be explained by the associative theories. In the first phase of a blocking design, a strong association is established between cue A and the outcome, leading the system to predict correctly the outcome whenever cue A is present. In phase 2, the outcome is also correctly predicted based on the association between cue A and the outcome alone. Therefore, no further updating of associations takes place, and therefore, the initial zero association between cue B and the outcome is not strengthened. When the system is later presented with cue B alone, it will not react because the formation of a strong association between cue B and the outcome has been "blocked." The introduction to Experiment 6 elaborates on associative theories and their account of the blocking effect.

Generalizability and Boundary Conditions

The context of consumers learning about brands and attributes differs from the traditional blocking paradigm in a number of ways. Some of the factors that distinguish consumer learning situations may act as moderators or boundary conditions of the blocking effect. It is, therefore, important to discuss these factors before continuing to the experiments.

Consumer beliefs. In traditional blocking studies, subjects have no pre-experimental beliefs about what cues are most likely to be important determinants of the outcome. However, when consumers learn to predict consumption outcomes, they do not come to the task as a blank slate. One type of beliefs that may impact consumer learning are beliefs about the causal roles of brands versus intrinsic attributes. Consumers may

believe that brands do not cause most consumption outcomes and function merely as a label or sign that refers to the intrinsic attributes that actually drive performance. One would not expect that pre-exposure to a sign of a potential cause would make that potential cause less likely to have caused an outcome. Instead, consumers may see the brand as a spurious predictor that is to be discarded as soon as information about a predictive intrinsic feature is received. In this case, no blocking should obtain.

The encoding and associative accounts of blocking cannot predict or explain such an effect of general causal beliefs. However, recent contingency accounts explicitly incorporate the influence of consumers' causal knowledge (e.g., Cheng 1997; Waldmann 1996; Waldmann and Holyoak 1992). For example, according to the causal model view of Waldmann and Holyoak (1992; see also Waldmann 1996), blocking and other so-called "cue competition" effects should depend heavily on people's causal beliefs and the conditional contingency analysis outlined above should not apply when the cues refer to potential outcomes instead of potential causes. Waldmann and Holyoak (1992) found that when the cues (A and B) in a blocking design were encoded as potential effects of the outcome instead of potential causes, no blocking effect was found. Thus, blocking may only occur when consumers perceive both predictive cues in a blocking paradigm as potential causes, e.g., when both cues are intrinsic attributes, but not when one of the cues refers to a non-causal predictor. Experiment 1 will address this issue by manipulating the causal nature of one of the cues.

In addition to general causal beliefs about the causal status of brand and attribute cues (cause versus sign), consumer learning about the predictive value of brands and attributes is often characterized by more specific causal beliefs about the likely importance of

specific attribute dimensions as determinants of specific consumption outcomes. For example, a consumer trying to learn to reliably predict the steering performance of whitewater rafts, may not know the actual types of rudders that are used on whitewater rafts, but it is likely that she will expect that steering performance should depend heavily on the rudder dimension. This type of specific causal beliefs favors learning of the importance of a critical attribute and may be expected to prevent blocking of that critical attribute. Experiment 2 will address this issue by manipulating specific causal beliefs.

Limited brand pre-exposure and absence of explicit prediction. The likely presence of different causal beliefs about the blocking cue (A) and the blocked cue (B) are not the only differences between many consumer learning contexts and the contexts in which blocking has previously been demonstrated. In the traditional human blocking paradigm, subjects receive a large number of pre-exposures (e.g., 48; Waldmann and Holyoak 1992) to the blocking cue (A). Also, within each trial, subjects predict whether the outcome will occur after presentation of the cue or cues, but before the outcome is presented. Thus, on each learning trial, the subject actively makes a prediction and receives immediate feedback about the outcome. This should lead to faster learning of the blocking cue (A) compared to the situation in which such active predictions are not made. However, it is not always the case that consumers receive a very large number of brand-only exposures before being exposed to attribute information. In addition, it is unlikely that consumers will make explicit quality predictions each time they receive product information. Thus, demonstrating a blocking effect with few pre-exposures and without explicit predictions at each experience would greatly enhance the generalizability of the effect to consumer learning situations.

Non-standard learning scenarios. Although many consumer learning situations map onto the standard "A+ -> AB+" blocking paradigm, other common consumer learning situations are structurally different. It is not clear whether blocking will obtain in these situations. For example, although many initial exposures to product information include information about just the brand and a consumption outcome (i.e., what it does or how good it is, A+), it is also common that a company consistently presents information about a "positioning attribute," to distinguish its product from other products and to give consumers a reason to believe that their product is superior to other products. In some cases, this positioning attribute is a unique attribute that directly enhances product performance. For example, ads for Pantene brand shampoo usually mention that it contains the Pro-Vitamin B-5 complex. In many cases, however, a positioning attribute is used that appears valuable, but is actually irrelevant to product performance (Carpenter, Glazer, and Nakamoto 1994). For example, Folger's instant coffee is advertised as having unique "flaked coffee crystals" implying that those crystals improve the taste of instant coffee. In reality, shape of crystals does not affect the taste of instant coffee (Carpenter et al. 1994). Whereas it could be expected that adding such an irrelevant attribute would increase the evaluation of products that possess it, it is unclear what it would do to the value consumers place on the brand name and what would happen to the blocking of a third, critical, attribute that does have a real impact on product performance. Experiment 3 addressed these questions by comparing blocking in this A_1A_2+ -> A_1A_2B+ scenario with the standard blocking scenario and the relevant control conditions.

Another consumer learning scenario that should be of interest is the scenario in which consumers are pre-exposed to performance information about multiple brands before

finding out that they share a common critical attribute. For example, the pain killers Advil, Motrin, and Nuprin share the same active ingredient, ibuprofen. According to Thagard (1989; see also Read and Marcus-Newhall 1993; Van Overwalle 1998), people should prefer explanations that are simpler and account for more data. To the extent that these criteria of "simplicity" and "breadth" extend to situations in which consumers try to determine the best predictor(s) of consumption outcomes, one would expect little blocking to occur when a simpler and broader predictor is presented in Phase 2 than the brands in Phase 1. Thus, the common attribute in the A_1+, $A_2+ \rightarrow A_1B+$, A_2B+ design introduced above would provide a "better explanation" for product performance that could replace the two separate "brand explanations." Experiment 7 addresses this issue by comparing blocking in the A_1+, $A_2+ \rightarrow A_1B+$, A_2B+ design with the effects in the standard blocking design and the relevant control conditions.

CHAPTER 3
EXPERIMENT 1

The most basic goal of Experiment 1 was to determine whether the blocking effect can be replicated with product features in a consumer context. In addition, Experiment 1 was intended to explore the influence of general causal beliefs that consumers might have about brands versus attributes as causes of product quality. I expected that general knowledge about the causal role of brand names would at least partially prevent the blocking of a critical attribute by pre-exposure to a brand name. However, when the first predictive feature that consumers learn is not a brand name but instead another intrinsic attribute, blocking should not be hampered by such causal beliefs. Whereas a brand name may be dismissed as a spurious predictor, the causal status of a pre-exposed attribute is the same as that of the critical attribute. Thus, I expected that the acquisition of value by a predictive critical attribute would be blocked more by pre-exposure to another predictive intrinsic attribute than by pre-exposure to a brand name. In fact, I expected to find an absence of blocking in the brand name case. In addition to the manipulation of the pre-exposed feature (brand name versus intrinsic attribute), the consumer learning context used in Experiment 1 differed from previous blocking experiments in at least two ways: (i) it employed a very small number of pre-exposures (four) and (ii) subjects did not make a prediction within each trial. These differences made Experiment 1 a very conservative test of the blocking effect.

Method

Overview. Forty subjects participated in this experiment. Subjects in this and all other experiments reported here were part of a subject pool of undergraduate students enrolled in an introductory marketing course who participated to obtain extra course credit. The basic paradigm used in Experiment 1 consisted of showing subjects a number of product descriptions including information about product features and information about the products' overall quality level. A schematic overview of the design of Experiment 1 is shown in Table 1. In the Experimental cells, subjects received 16 learning trials, each consisting of one product description, divided across two learning phases. Learning Phase 1 consisted of four trials. The product descriptions in Learning Phase 1 consisted of information about (i) the quality level of the products, (ii) a first predictive feature (A), and (ii) two filler attributes that were not correlated with quality. In Learning Phase 2, information about a second predictive feature, the critical attribute (B), was added. The levels of this attribute were perfectly correlated with those of the first predictive feature and with quality level. After inspecting all the products, subjects were presented with additional product descriptions. This time, the two predictive features (A and B) were varied orthogonally. Subjects were asked to judge the quality levels of the additional products and to indicate their willingness-to-pay for those products. In addition, subjects rated the extent to which each feature predicted quality in the product category. The only difference between the Control cells and the Experimental cells was that Learning Phase 1 was omitted in the Control cells. The first predictive feature was either a brand name or one of four intrinsic attributes. The positions of the predictive features in the product descriptions (the first predictive feature

at the top and the second at the bottom versus the second predictive feature at the top and the first at the bottom) were counterbalanced between subjects to control for position effects. In sum, the basic experimental design was a 2 (Experimental versus Control) * 2 (Type of Predictive Feature: Brand versus Attribute) * 2 (Position) completely randomized factorial design.

TABLE 1
EXPERIMENT 1: DESIGN SUMMARY

Type of First Predictive Feature / Experimental vs. Control	Experimental Phase: Learning 1	Learning 2	Test
Brand			
Experimental	$A_{br1}+$	$A_{br1}B_{ca1}+$	$A_{br}?$
	$A_{br2}-$	$A_{br2}B_{ca2}-$	$B_{ca}?$
Control		$A_{br1}B_{ca1}+$	$A_{br}?$
		$A_{br2}B_{ca2}-$	$B_{ca}?$
Attribute			
Experimental	$A_{at1}+$	$A_{at1}B_{ca1}+$	$A_{at}?$
	$A_{at2}-$	$A_{at2}B_{ca2}-$	$B_{ca}?$
Control		$A_{at1}B_{ca1}+$	$A_{at}?$
		$A_{at2}B_{ca2}-$	$B_{ca}?$

Note: Abbreviations used are "br" for "brand," "at" for "attribute," "ca" for "critical attribute," "+" for "high quality," and "-" for "low quality." Subscripts "1" and "2" refer to different levels of a feature.

Procedure and stimuli. Subjects were run in groups of one to five and were randomly assigned to experimental conditions. Each subject was seated at a separate computer that was running the Micro Experimental Laboratory software system (Schneider,1988). After receiving oral instructions about the computer setup, the monitor displayed instructions informing subjects about the types of stimulus information they would receive and asking them to try to "learn how to predict the quality level of whitewater rafts" and to "determine what really matters in whitewater rafts" (see Appendix A for the

instructions to this and other experiments). Next, subjects were presented with 16 (Experimental cells) or 12 (Control cells) profiles of whitewater rafts that they inspected at their own pace (see Appendix B for schematic representations of profiles in this and other experiments). Each profile contained a random catalog-style identification number, information about three or four product features (a brand name plus attributes or attributes only), and information about the quality level of the whitewater raft (high versus low). In the Experimental cells but not in the Control cells, subjects were first presented with profiles for four rafts that included information about the first predictive feature and about two filler attributes. The first predictive feature was either a brand name or one of four attributes ("steering," "bow," "frame," "outer skin") and had two levels that were perfectly predictive of the raft's quality level. Rafts that had a "hypalon" brand name, steering system, bow, frame, or outer skin were always high quality whereas rafts that had the "riken" level were always low quality rafts. Each level was used for two of the rafts. For half the subjects in the Experimental cells, the first predictive feature was presented at the top of the three-item list of features. For the other half of the subjects, the first predictive feature was placed at the bottom. One of the filler features was a constant feature. All rafts had a "polyurethane hull." The second filler feature had two levels, but was uncorrelated with quality. Two of the rafts had "airecell compartments." The other two had "closed-cell compartments." After the presentation of the four rafts in this first learning phase, subjects relaxed for 10 seconds, were instructed to think about the rafts they had seen, and were encouraged to pay attention again when looking at the upcoming second set of raft profiles. Next, in a second learning phase, subjects received 12 additional profiles. In these profiles the same features were

presented in the same proportions and in the same predictive or unpredictive relationships with quality levels as in the first learning phase. In this second learning phase, information about a fourth feature, "floor," was added. The information about this second predictive feature, the critical attribute, was positioned at the top of the feature lists for subjects who were receiving the first predictive feature at the bottom and at the bottom for subjects who were receiving the first predictive feature at the top of the profiles. The critical attribute was perfectly correlated with both the first predictive feature and the quality levels. All six high quality rafts had a "tubular" floor and all six low quality rafts had an "i-beam" floor. After Learning Phase 2, the computer instructed subjects to open a paper and pencil answer booklet and responded to the dependent measures. The only treatment difference between the Control cells and those in the Experimental cells is that the former bypassed Learning Phase 1. Thus, Control subjects were not pre-exposed to the first predictive feature before receiving information about the second predictive feature.

Dependent measures. In the answer booklet (see Appendix C for examples of the dependent measures used in this and other experiments), subjects were first presented with eight additional profiles of rafts giving information about all four features. The eight profiles were constructed as a 2 * 2 * 2 factorial combination of the two levels of the first predictive feature, the varying but uncorrelated feature, and the second predictive feature. Thus, four of the profiles were identical to profiles seen before. The remaining four profiles differed from the first four items in that the level of the first predictive feature that had been paired with high quality was now presented in a profile together with the low quality level of the second predictive feature and vice versa. For each of the

eight rafts, subjects indicated whether the quality level of the raft was high or low.[1] In the second measure, subjects were again presented with four-feature profiles. This time, the levels of both filler features were held constant. The nine profiles were constructed as a 3 * 3 factorial combination of the two levels of the two predictive features presented in the second learning phase plus a not previously presented level ("ensolite" brand name, steering system, bow, frame, or outer skin and "batten" floor). Subjects were asked to guess the price that they thought would be reasonable for each raft. They were told that prices ranged from $1,500 to $2,500 with an average of $2,000. Finally, subjects were asked to rate each of the features (brand/steering/bow/frame/outer skin, compartments, hull, and floor) independently as to whether it predicted the quality level of rafts on a 21-point scale ranging from -10: Definitely does not predict quality level, over 0: Don't know, to +10: Definitely predicts quality level.[2]

Results

To summarize briefly, results from the three dependent measures all showed a strong blocking effect that was not significantly dependent on the nature of the first predictive feature (Brand versus Attribute) or on the position of the first predictive feature in the

---

[1] In addition to indicating the quality level, high or low, subjects also rated their confidence about each quality level judgment for each profile. Results for this measure showed little variance and high means, indicating that subjects were supremely confident across all profiles and conditions. An additional dependent measure was constructed by multiplying the confidence rating and the quality judgment (1 for high quality, -1 for low quality) for each profile. An ANOVA on this dependent measure yielded the same results as the logistic regressions on the quality judgments only. The only potentially relevant exception was an unexplainable but statistically significant five-way interaction between level of the first predictive feature, level of the uncorrelated filler feature, pre-exposure versus simultaneous exposure, type of first predictive feature, and position of the predictive features in the profiles.

[2] The answer booklet also included a number of post-experimental questions. Results for these questions indicated that (i) subjects' self-reported knowledge about whitewater rafts was invariably low, (ii) no suspicions were raised about the truthfulness of the product information, and (iii) subjects could not guess the hypotheses.

product profiles (presented at the top or at the bottom of each profile). Two of the three dependent measures also showed a positive effect of pre-exposure on the value subjects placed on the first predictive feature.

TABLE 2
EXPERIMENT 1: MEAN FEATURE EFFECTS ON BINARY QUALITY JUDGMENTS

	Mean Feature Effects		
Type of 1st Predictive Feature / Feature	Control	Experimental	Difference
Brand			
Brand (A)	.15	.70	.55
Floor (B)	.60	.20	-.40
Attribute			
Attribute (A)	.35	.75	.40
Floor (B)	.45	.20	-.25

Binary quality judgments. The results for the measure using eight profiles with binary quality judgments were first analyzed using two logistic regression models that each included the levels of one of the two predictive features, of the two main between-subjects independent variables and of the Position counterbalancing factor plus all simple and higher-order interactions. Because no statistically significant effects of the position of features in the profiles were found in these models (all *p*s > .10), two additional models were run in which Position and its interactions were omitted. Simple contrast analyses also excluded Position and its interactions.

Because, all hypotheses are at the level of the feature dimension, the theoretical dependent variable is the difference in evaluations between products that had one level of a feature and products that had the other level of the feature. For example, to assess the value a consumer places on the brand feature, one has to look at the difference in

evaluation between products that carried the Hypalon brand name and products carrying the Riken brand name. For all practical purposes, this difference can be interpreted as a brand part-worth or a brand weight and will be referred to as a "feature effect" in the Tables. Table 2 shows a summary of the mean feature effects after collapsing the results across levels of Position. The mean feature effects were computed by taking the difference in percentage of profiles judged to be high quality between the profiles with one level of a feature and the profiles with the other level of that feature. For example, when subjects learned about the two predictive features simultaneously (Control) and the first predictive feature was a brand name, subjects judged 85% of the profiles with a tubular floor to be high quality versus only 25% of the profiles with an i-beam floor. Thus, the mean feature effect of floor in this cell equaled .60 (.85 - .25).

According to the "general causal beliefs" hypothesis, the blocking effect should be larger when the first predictive feature (A) refers to brand than when it refers to an intrinsic attribute. In other words, the difference between the mean feature effect of the critical attribute (B) in the simultaneous exposure condition (Control) and the same mean feature effect in the pre-exposure condition (Experimental) should be larger when the first predictive feature (A) referred to another attribute than when it referred to brand name. The binary quality judgment data do not support this hypothesis but indicate a strong overall blocking effect (asymptotic $t = -3.51, p < .001$). As expected, when the first predictive feature (A) was an attribute, the mean feature effect (*MFE*) of the critical attribute (B) was larger in the simultaneous exposure scenario ($MFE = .45$) than in the pre-exposure scenario ($MFE = .20$), a difference of .25 (asymptotic $t = -1.85, p < .1$). However, this difference was not smaller when the first predictive feature (A) referred to

brand name. In fact, the mean feature effect of the critical attribute (B) was .60 in the simultaneous exposure condition versus only .20 in the pre-exposure condition, a difference of .40 (asymptotic $t = -3.51, p < .001$). This interaction effect was not statistically significant (asymptotic $t = 1.39$, n.s.).

The pattern of mean feature effects of the first predictive feature (A) mirrors the pattern for the critical attribute (B). An overall effect of pre-exposure was found, indicating that the first predictive feature (B) was weighted more heavily when it was pre-exposed than when it was presented simultaneously with the critical attribute (asymptotic $t = 2.82, p < .01$). The mean feature effects suggest that the pre-exposure effect was larger when the first predictive feature referred to brand name (*MFE*s .15 for simultaneous exposure, .70 when pre-exposed, a difference of .55; asymptotic $t = 2.82, p < .01$) than when it referred to an attribute (*MFE*s .35 for simultaneous exposure, .75 when pre-exposed, a difference of .40; asymptotic $t = 2.66, p < .01$). However, this interaction was not statistically significant (asymptotic $t = -.09$, n.s.).

Although the interaction effects supporting such an interpretation were not statistically significant, the overall pattern of means suggests that the subjects did have general causal beliefs, but that these causal beliefs were "overwhelmed" by the effects of pre-exposure. When subjects learned about the two predictive features simultaneously and the first predictive referred to brand name, the critical attribute (floor) was weighted heavily in quality judgments (*MFE* = .60) whereas brand name had almost no impact (*MFE* = .15). When subjects learned simultaneously about the two predictive features and both were intrinsic attributes, both attributes had similar medium-sized effects (*MFE*s of .45 for the critical attribute and .35 for the first predictive feature). Thus, the pattern of

means was consistent with the existence of general causal beliefs favoring intrinsic attributes over brands as indicators of product performance. When subjects were pre-exposed to brand information or information about one of four intrinsic attributes, the pre-exposed features received large weights (*MFE*s .70 for brand and .75 for attributes) and the critical attribute received a very small weight (*MFE*s .20 after pre-exposure to brand and .20 after pre-exposure to another intrinsic attribute), regardless of whether the pre-exposed feature referred to brand or an intrinsic attribute.

TABLE 3
EXPERIMENT 1: MEAN FEATURE EFFECTS ON NORMALIZED WILLINGNESS-TO-PAY JUDGMENTS

	Mean Feature Effects		
Type of 1st Predictive Feature / Feature	Control	Experimental	Difference
Brand			
Brand (A)	.83	1.28	.45
Floor (B)	1.51	.43	-1.08
Attribute			
Attribute (A)	1.02	1.81	.79
Floor (B)	1.32	.67	-.66

Willingness-to-pay judgments. Given the typically large individual differences in scale use for "open-ended" willingness-to-pay (WTP) questions, prices were normalized for each individual separately by subtracting the subject's mean price from each price and dividing the result by the standard deviation computed on the basis of the nine prices given by that subject.[3] The nine willingness-to-pay judgment were first analyzed using an omnibus ANOVA. The critical interactions (of each of the two predictive features with Experimental versus Control and Type of First Predictive Feature) were not

[3] Analysis of the untransformed data showed exactly the same effects. See Appendix D for raw data

qualified by significant higher-order interactions. Planned contrasts were then performed that included the linear trend of the levels of one of the two predictive features,[4] Experimental versus Control, and Type of Predictive Feature as predictors.

Table 3 shows the mean feature effects for the WTP measure collapsed across levels of Position. The mean feature effects presented in Table 3 were computed by subtracting the price judgments for products that had the level of the feature that had been coupled with low quality in the stimuli from the price judgments for the products that had the level that had been coupled with high quality. For example, in each condition, the floor effect was computed by subtracting the prices subjects were willing to pay for rafts that had an i-beam floor from the prices they were willing to pay for rafts with a tubular floor.

Results for the WTP measure again showed a strong blocking effect as indicated by a significant simple interaction between the Experimental versus Control factor and the linear trend of the three levels of the critical attribute ($F_{1,36} = 12.66, p < .01$). In other words, the level of the critical attribute (i.e., the type of floor) had more impact on subjects' relative willingness-to-pay for a whitewater raft when they received information about the two predictive features simultaneously than when they received information about the critical attribute after just four pre-exposures to a brand name or another intrinsic attribute. A significant simple interaction was also obtained between the Experimental versus Control factor and the linear trend of the three levels of the first predictive feature ($F_{1,36} = 7.34, p < .02$), indicating an increased effect of the first predictive feature when it was pre-exposed instead of learned simultaneously with the

---

means.

[4] Linear trend coefficients for both predictive features were -1 for the level that had been coupled with low quality during learning, 0 for the new level, which had not been presented during learning, and +1 for the

critical attribute. These effects were not qualified by a statistically significant higher-order interaction with Type of First Predictive Feature (*F*s < 1). Thus, the blocking effect was not significantly moderated by the nature of the first predictive feature (brand name versus intrinsic attribute).

Traditionally, the effect referred to as "blocking" is the decrease in predictive value of the second predictive feature (B cue), in this case the critical attribute, because of pre-exposure to a first predictive feature (A cue). Blocking is usually not used to refer to the effect of pre-exposure on the A cue. In the binary quality judgments measure the reduced mean feature effect of the B cue (critical attribute) in the Experimental conditions may be due entirely to the fact that its relative predictive value decreased compared to the pre-exposed A cue (brand or another attribute), while the "absolute" predictive value of the B cue was not affected. For example, in some of the binary quality judgment profiles, subjects had to indicate whether a raft that had the Hypalon brand name (coupled with high quality in the learning phases) and an i-beam floor (coupled with low quality during learning) was high or low quality. It is possible that although subjects in the Experimental conditions clearly viewed the i-beam floor as detrimental to the quality of the product, it would still just make the cutoff to be called high quality, because the four extra exposures to the brand name information caused brand to be weighted more heavily than in the control conditions. Such an effect, due entirely to an increase in the predictive value of the first predictive feature with no decrease in the predictive value of the critical attribute, should disappear with more continuous measures such as the willingness-to-pay measure, because these measures allow simultaneous independent expression of both

---

level that had been coupled with high quality.

effects. For example, a Hypalon brand raft with an i-beam floor can be valued more than the average raft, but still less than a Hypalon brand raft with a tubular floor. The finding of a strong effect on the mean feature effect of the critical attribute (floor) with (raw and normalized) willingness-to-pay judgments suggests that the phenomenon found here is genuine blocking.

TABLE 4
EXPERIMENT 1: NORMALIZED WILLINGNESS-TO-PAY JUDGMENTS AT NEW LEVEL OF FIRST PREDICTIVE FEATURE

	Mean Feature Effects[a]		
Type of 1st Predictive Feature	Control	Experimental	Difference
Brand	1.55	.51	-1.04
Attribute	.95	.36	-.59

[a]Differences in prices between leafield rafts with a tubular floor versus an i-beam floor.

Nevertheless, if the additional assumptions is made that subjects use only one cue as input into their WTP judgment, even the results for judgments on a more continuous scale could be explained by a relative effect of pre-exposure on the predictive value of the first predictive feature. For example, the diagnosticity-accessibility framework by Feldman and Lynch (1988; see also Alba, Hutchinson, and Lynch 1991; Lynch, Marmorstein, and Weigold 1988) could account for such an effect. According to Lynch and his colleagues, consumers will base decisions on the most accessible input that exceeds a given level of perceived diagnosticity. It would be possible that subjects in all conditions learned that both features were diagnostic as predictors of the outcome, but that pre-exposure to one cue (e.g., a brand name) made information about that cue more accessible without influencing the perceived diagnosticity and accessibility of other cues (e.g., levels of the critical attribute). Thus, when judging the quality of a profile that contains known levels

of both predictive features, subjects in the Experimental condition may have based their decisions solely on the level of the first predictive feature (e.g., the brand name), because quality information about that input was most easily accessible.

However, such a diagnosticity-accessibility account would make different predictions for situations in which subjects were asked to give an independent judgment about the critical attribute or for situations in which a new level of the first predictive feature was used about which no quality information was present in memory. In such a situation, only the information about the levels of the critical attribute should be diagnostic and accessible. Thus, if pre-exposure had not affected learning of the predictive value of the critical attribute, the effect of pre-exposure on the mean feature value of the critical attribute should disappear in these situations. An analysis of the willingness-to-pay data for the profiles with a new, unknown level of the first predictive feature (see Table 4) provided clear evidence against the diagnosticity-accessibility account.[5] Despite the absence of diagnostic information about the first predictive feature, a strong effect of pre-exposure was found on the dollar premium subjects were willing to pay for a tubular floor versus an i-beam floor, as indicated by the interaction between the linear trend of the levels of the critical attribute and the Experimental versus Control factor ($F_{1,36} = 5.71, p < .05$). This effect was not qualified by a significant higher-order interaction with Type of First Predictive Feature ($F < 1$). These results suggest that the results found in the binary quality judgments cannot be explained solely in terms of the effect of pre-exposure on the

---

[5] The analysis reported here was performed on the transformed willingness-to-pay data. Analysis of the untransformed data showed exactly the same effects. See Appendix D for raw data means. Results in Appendix D show that subjects believed that the reasonable premium for a "generic" raft with Tubular Floor over a generic raft with an I-Beam Floor was about three times lower when they had previously received a very limited number of pre-exposures to brand name information.

predictive value of the pre-exposed feature, but that they reflect a classic blocking effect on the predictive value of the critical attribute.

TABLE 5
EXPERIMENT 1: MEAN FEATURE PREDICTIVENESS RATINGS

	Mean Feature Effects		
Type of 1st Predictive Feature / Feature	Control	Experimental	Difference
Brand			
Brand (A)	6.0	5.6	-.4
Floor (B)	9.1	6.1	-3.0
Attribute			
Attribute (A)	8.7	9.1	.4
Floor (B)	8.1	3.5	-4.6

Predictiveness ratings. The predictiveness ratings for the critical attribute provided another independent measure of the blocking effect that should not be due to a relative increase in the predictive value of the first predictive feature as a result of extra exposures in the first learning phase. The predictiveness ratings for the two predictive features were analyzed using two 2 (Experimental versus Control) * 2 (Brand versus Attribute) * 2 (Position) ANOVAs on the predictiveness ratings of the two predictive features (see Table 5 for a summary of means). Because no effects of Position were found, two additional ANOVAs and simple contrasts were computed collapsing across the Position counterbalancing factor. Consistent with the other two dependent measures, the results for the ratings showed that subjects were more certain that the critical attribute was predictive of product quality when they were exposed to information about both predictive features simultaneously (mean rating = 8.60) than when they received that information after just four pre-exposures to the first predictive feature (mean rating = 4.80; $F_{1,36} = 7.31, p < .02$). None of the other effects was significant (ps > .20). Simple

contrasts indicated a statistically significant effect of pre-exposure when the first predictive feature referred to brand names ($F_{1,18} = 6.34, p < .05$) but not when the first predictive feature referred to another attribute ($F_{1,18} = 3.26, p < .10$).

Unexpectedly, the results for the first predictive feature were not consistent with the results for the other dependent measures. No effect of the pre-exposure manipulation on the predictiveness rating for the first predictive feature was found ($F < 1$). When the first predictive feature referred to brand name, the absence of a pre-exposure effect seemed to be due to an unexpectedly low predictiveness rating in the Experimental condition. The low mean rating could be attributed partially to one outlying data point in a total of just 10 data points. The one outlying rating of -10 on the 21 point scale contrasted with a mean rating of 7.33 for the other nine subjects. The absence of an effect of pre-exposure on the predictiveness of the first predictive feature when that feature referred to an attribute could be explained by the high predictiveness rating for the first predictive feature in the control condition, leading to a ceiling effect. Consistent with the assumption that consumers have general causal beliefs favoring intrinsic attributes over brands as indicators of product performance, results indicated that the first predictive feature was judged to be predictive of quality with more certainty when it referred to an attribute than when it referred to brand name ($F_{1,36} = 7.91, p < .01$).

Instead of interpreting the ratings by comparing results between Control and Experimental conditions within each predictive feature separately, the predictiveness ratings data can also be analyzed by looking at the differences in predictiveness ratings between the two predictive features and the interaction of this difference with the pre-exposure factor (Control versus Experimental conditions). Results for such an analysis

collapsing across Position showed a marginally significant interaction indicating that the "advantage" in perceived predictiveness of the critical attribute decreased after pre-exposure to the first predictive feature ($F_{1,36} = 3.70, p < .10$). This effect was not qualified by a higher-order interaction with Type of First Predictive Feature ($F < 1$). Thus, the effect of pre-exposure was not significantly different when the first predictive feature referred to another attribute than when it referred to brand names. There was, however, a significant interaction of the difference in perceived predictiveness between the critical attribute and first predictive feature with the Type of Predictive Feature factor ($F_{1,36} = 6.15, p < .02$). Consistent with the idea that consumers have general beliefs about brands and attributes as reliable predictors of product performance, this interaction indicated that the first predictive feature was thought to be less likely to predict quality relative to the critical attribute when that first predictive feature referred to brand names than when it referred to another intrinsic attribute. The lack of significance of the three-way interaction, however, suggested that this effect is independent of the effect of pre-exposure.

Discussion

Experiment 1 suggests that providing consumers with diagnostic information about a critical attribute does not necessarily cause consumers to value this critical attribute at the expense of the brand name. The data do suggest that when consumers learn about the brand name and the critical attribute at the same time, such an "equity shift" does occur. In fact, the binary quality judgments in the first conjoint-type measure showed that brand names retained very little predictive value in this simultaneous learning case. However, when subjects had just four experiences with the brand names before they were exposed

to the critical attribute information, the critical attributes gained little predictive value, and a very strong brand preference was observed. Thus, a strong "blocking" effect was observed in which prior learning of one predictive cue inhibited the acquisition of predictive value by subsequently presented equally predictive cues. Contrary to the general causal beliefs hypothesis, this blocking effect was not smaller when the pre-exposed cue referred to brand names than when it referred to intrinsic attributes. There were, however, some signs that subjects had beliefs about the role of brands versus attributes as predictors of product performance and that these beliefs influenced quality predictions, price judgments, and predictiveness ratings. This influence seemed to be limited mostly to situations in which both predictive features were learned simultaneously. Except in the predictiveness ratings, pre-exposure seemed to overwhelm any impact of prior beliefs.

Although Experiment 1 suggests that the blocking effect is not diminished by general beliefs favoring attributes over brands as causes or predictors of consumption outcomes, findings by Broniarczyk and Alba (1994b) suggest strong effects of more specific consumer prior beliefs on quality predictions. For example, Broniarczyk and Alba (1994b) found that prior beliefs about price-quality relationships strongly influenced consumers' predictions of quality even when price and quality were actually uncorrelated and the data showed a significant correlation with quality for another predictor that was not favored by specific prior beliefs. Thus, it might be expected that if the critical attribute in a blocking design has a strong theoretical relation to the outcome and that theoretically related cue is equally predictive of the outcome as a brand name (instead of less predictive in the Broniarczyk and Alba case), consumers should base their quality

predictions on the theoretically related attribute, regardless of pre-exposure to the brand name. In addition, using a theoretically related critical attribute should make it more likely that consumers would interpret the brand name as being a label that merely refers to that underlying critical attribute, thereby reducing the blocking effect.

In addition to specific priors favoring the critical attribute over the brand, the strong blocking effect found in Experiment 1 might be decreased by increasing the number of trials in the second learning phase and by using extrinsic instead of intrinsic filler features. Increasing the number of exposures to the critical attribute in the second learning phase should make it easier for subjects to learn the predictive relationship between the critical attribute, despite the interference caused by the four pre-exposure trials. Using extrinsic (e.g., country-of-origin, warranty) instead of intrinsic (e.g., compartments, hull) filler features might make the critical attribute stand out as the only intrinsic attribute available. Also, whereas in Experiment 1 the critical attribute may have competed for attention with the two potentially directly causal filler features, extrinsic filler features might compete more with the brand because they might also be interpreted as signs or non-causal predictors. Experiment 2 provided further tests of the robustness of the effects of pre-exposure found in Experiment 1 by addressing the issues of specific prior beliefs, number of exposures to the critical attribute, and the use of extrinsic features.

CHAPTER 4
EXPERIMENT 2

The main focus in Experiment 2 was to assess the influence on the blocking effect of specific prior beliefs regarding the critical attribute and the consumption outcome. In addition to testing the robustness of the blocking effect in the face of specific prior beliefs, Experiment 2 tested the robustness of the effects found in Experiment 1 by manipulating the number of second phase learning trials (12 versus 24 trials) and by using extrinsic uncorrelated and constant features.

Method

One hundred-and-eleven subjects participated in groups of one to eight persons. The procedure and stimulus format in Experiment 2 were similar to those used in Experiment 1. In Experiment 2, however, some of the features and feature levels were changed (see Appendix B). For all subjects, the first predictive feature was brand, with levels Hypalon and Riken. As in Experiment 1, Hypalon was coupled with a good consumption outcome and Riken was paired with a bad consumption outcome. The constant filler feature was warranty, which was one year for all rafts. The uncorrelated filler feature was the country of origin ("made in"), which was either "Sweden" or "Canada." Finally, the critical attribute was the raft's "rudder," which was "fourcase" for the high quality rafts and "backbar" for the low quality rafts. The critical attribute was again perfectly coupled with the brand name and with the consumption outcome. For half the subjects (those in the

Low Prior Beliefs cells), the consumption outcome information was information about the raft's general quality level ("high quality" versus "low quality"), an outcome with little a priori relatedness with the critical attribute (rudder). For the remaining subjects (those in the High Prior Beliefs cells), the outcome pertained to the steering quality of the rafts ("good steering performance" versus "bad steering performance"). In the second learning phase, half the subjects received 12 learning trials and the other half received 24 learning trials. Thus, the basic design of Experiment 2 was a 2 (Experimental vs. Control) * 2 (Number of Phase 2 Trials) * 2 (Prior Beliefs) completely randomized factorial design. See Table 6 for a design summary.

TABLE 6
EXPERIMENT 2: DESIGN SUMMARY

	Experimental Phase		
Prior Beliefs/ Experimental vs. Control	Learning 1	Learning 2	Test
Low Prior Beliefs			
Experimental	$A_{br1}+_{lp}$	$A_{br1}B_{ca1}+_{lp}$	$A_{br}?$
	$A_{br2}-_{lp}$	$A_{br2}B_{ca2}-_{lp}$	$B_{ca}?$
Control		$A_{br1}B_{ca1}+_{lp}$	$A_{br}?$
		$A_{br2}B_{ca2}-_{lp}$	$B_{ca}?$
High Prior Beliefs			
Experimental	$A_{br1}+_{hp}$	$A_{br1}B_{ca1}+_{hp}$	$A_{br}?$
	$A_{br2}-_{hp}$	$A_{br2}B_{ca2}-_{hp}$	$B_{ca}?$
Control		$A_{br1}B_{ca1}+_{hp}$	$A_{br}?$
		$A_{br2}B_{ca2}-_{hp}$	$B_{ca}?$

Note: Abbreviations used are "br" for "brand," "ca" for "critical attribute," "$+_{lp}$" for "positive outcome, unrelated to critical attribute," "$-_{lp}$" for "negative outcome, unrelated to critical attribute," "$+_{hp}$" for "positive outcome, related to critical attribute," "$-_{lp}$" for "negative outcome, related to critical attribute." Subscripts "1" and "2" refer to different levels of a feature. Half the subjects received 6 trials and the other half of the subjects received 12 trials of each type in Learning Phase 2.

Dependent measures in Experiment 2 were simplified. First, subjects indicated their binary quality judgments using a response format equivalent to the first measure in

Experiment 1. Next, subjects were presented with three pairs of raft profiles. In each pair, one of the rafts was a Riken brand raft that was made in Canada, with a one year warranty, a backbar rudder, and a price of $2000. The second raft was identical except that it had the other level of one feature (Hypalon brand, made in sweden, or fourcase rudder) and the absence of a price. After being informed that prices on the market range from $1500 to $2500, subjects were asked to indicate for each pair how much more or less they were willing to pay for the second raft compared to the first raft. Thus, subjects were asked to indicate the dollar premium they were willing to pay for one level over the other of the brand name, the uncorrelated but varying filler feature (country of origin), and the critical attribute (rudder).

Results

Overall, no support was obtained for the hypothesis that the blocking effect would disappear when (1) consumers have strong prior beliefs about the relevance of the critical attribute, (2) consumers have greater opportunity to learn in the second learning phase, or (3) no intrinsic filler features are used. As in Experiment 1, subjects placed significantly less value on the critical attribute and more on the brand name when they learned about the brand name first than when they learned about both predictive features simultaneously. This blocking effect was not significantly affected by either the specific prior beliefs manipulation or the number of Phase 2 trials.

Binary quality judgments. The results for the binary quality judgment measure were analyzed using two logistic regression models that each included the levels of one of the two predictive features and of the three between-subjects independent variables plus all the simple and higher-order interactions between the predictive feature and the

between-subjects variables. Table 7 shows the mean feature effects for the two predictive features.

TABLE 7
EXPERIMENT 2: MEAN FEATURE EFFECTS ON BINARY QUALITY JUDGMENTS

	Mean Feature Effects								
	Control			Experimental			Difference		
Prior Beliefs / Feature	12	24	All	12	24	All	12	24	All
Low Prior Beliefs									
Brand (A)	.31	.43	.36	.77	.75	.76	.46	.32	.39
Rudder (B)	.69	.43	.57	.10	.14	.12	-.59	-.29	-.45
High Prior Beliefs									
Brand (A)	.19	.21	.20	.41	.57	.49	.22	.36	.29
Rudder (B)	.81	.71	.76	.38	.43	.40	-.43	-.28	-.36

The results for the mean feature effect of the critical attributes showed a significant blocking effect of brand name pre-exposure on the effect of rudder (asymptotic $t = -2.41$, $p < .02$) on quality judgments. The analysis also yielded a significant effect of the specific prior beliefs manipulation (asymptotic $t = -2.46$, $p < .05$) indicating that subjects put more weight on the rudder attribute when the outcome was steering performance than when the outcome was overall quality. This effect can be interpreted as a successful manipulation check on the Prior Beliefs manipulation. Interestingly, no support was found for the hypothesis that the blocking effect would be moderated by specific prior beliefs (asymptotic $t = .47$, n.s.). In addition, this analysis yielded no significant effects of the number of trials in Learning Phase 2 (all *ps* >.25). To provide a further test of the robustness of the blocking effect, simple contrasts measuring the effect of pre-exposure on the feature effect of the critical attribute for each of the four combinations of the other

two between-subjects variables (Prior Beliefs and Number of Phase 2 Trials) were computed. All four contrasts were statistically significant (*p*s $< .05$).

Thus, these results showed a significant and robust blocking effect. No significant decrease in blocking was found when subjects had specific prior beliefs favoring the critical attribute or when the number of exposures to the critical attribute was doubled from 12 to 24. However, a significant overall effect of the Prior Beliefs manipulation was found, indicating that subjects did place considerable value on the critical attribute even after brand pre-exposure when specific prior beliefs favor the critical attribute. Although the actual blocking effect (i.e., the difference in predictive value of the critical attribute between the Experimental and Control conditions) was not significantly affected by specific prior beliefs, they did reduce the absolute level of "blockedness" of the critical attribute after pre-exposure (asymptotic $t = 2.33, p < .02$).

The results for the effect of brand on quality judgments generally mirrored the results for the critical attribute. Just four pre-exposures to brand information caused a large increase in the mean feature effect of brand (asymptotic $t = 2.79, p < .01$). The pre-exposure effect was not qualified by a statistically significant interaction with Prior Beliefs (asymptotic $t = .36$, n.s.) or any other higher-order interactions. No statistically significant effects were found of the manipulation of the number of Phase 2 trials (all *p*s > .30). The general effect of the Prior Beliefs manipulation showing a decreased impact of brand on quality judgments when the outcome was related to the critical attribute was not statistically significant (asymptotic $t = 1.60, p = .11$). The contrast measuring the effect of pre-exposure on the brand feature effect was not statistically significant at the .05 level in the cells with High Prior Beliefs and 12 Phase 2 trials (asymptotic $t = 1.72, p < .10$), but

it was significant for the three other combinations of Prior Beliefs and Number of Phase 2 Trials ($ps < .01$).

TABLE 8
EXPERIMENT 2: FREQUENCIES OF SUBJECTS SHOWING QUALITY JUDGMENT PATTERNS

	Frequencies (Proportions)			
Judgments Depending On:	High Prior Beliefs		Low Prior Beliefs	
	Experimental	Control	Experimental	Control
Brand (A)	14 (.50)	4 (.16)	24 (.73)	8 (.33)
Rudder (B)	11 (.39)	17 (.68)	5 (.15)	12 (.50)
Other[a]	3 (.11)	4 (.16)	4 (.12)	4 (.17)

[a]"Other" patterns are driven by both Brand and Rudder and/or the uncorrelated filler feature (country of origin).

The cell sizes in Experiment 2 also allowed for an analysis of the results from the binary quality judgments measure by looking at frequencies of judgment patterns at the subject level. If pre-exposure to the brand name completely blocks the acquisition of value by the critical attribute, then more subjects in the Experimental condition than in the Control condition should judge all four Hypalon brand rafts in this measure to be high quality, and they should classify all four Riken brand rafts to be low quality, regardless of the level of the critical attribute. In other words, they should show a classification pattern that is completely driven by the brand names, the first predictive feature. Frequencies of classification patterns as a function of prior beliefs and pre-exposure are presented in Table 8. A logistic regression was performed on the classification patterns with the between-subjects factors and their interactions as independent variables. The dependent variable had two levels, "completely brand-driven" versus "not completely brand-driven." The results for this subject-level (instead of item-level) analysis again showed a

statistically significant blocking effect (asymptotic $t = -2.01$, $p < .05$). This effect was not qualified by statistically significant higher-order interactions, and no effects of Prior Beliefs or Number of Phase 2 Trials reached statistical significance (*p*s > .25).

TABLE 9
EXPERIMENT 2: MEAN FEATURE EFFECTS ON NORMALIZED WILLINGNESS-TO-PAY JUDGMENTS

	Mean Feature Effects								
	Control			Experimental			Difference		
Prior Beliefs / Feature	12	24	All	12	24	All	12	24	All
Low Prior Beliefs									
Brand (A)	.27	.26	.26	.73	.65	.68	.46	.39	.42
Rudder (B)	.41	.50	.46	.13	.16	.14	-.28	-.34	-.32
High Prior Beliefs									
Brand (A)	-.21	-.25	-.23	.59	.29	.44	.80	.54	.67
Rudder (B)	.71	.74	.72	-.09	.11	.01	-.80	-.63	-.71

Willingness-to-pay measure. As in Experiment 1, subjects varied widely in terms of the range of premiums they were willing to pay, with some subjects assigning premiums of approximately \$100 and others assigning premiums of up to \$1000 (i.e., going outside the range given in the instructions). Ten subjects failed to indicate their premiums in an unambiguous way. Data from these subjects were discarded from the analyses for this measure. To solve the scale-use problem, the three premiums again were normalized for each subject separately by subtracting the subject's mean premium from each premium and dividing the result by the standard deviation computed on the basis of the three premiums given by that subject. Mean feature effects are presented in Table 9. A 2 (Experimental versus Control) * 2 (Number of Phase 2 Trials) * 2 (Prior Beliefs) between-subjects ANOVA on the normalized premiums for the critical attribute (floor) yielded only one significant effect, i.e., the blocking effect. The premium that subjects

were willing to pay for the level of the critical attribute that was paired with high quality was lower in the Experimental condition than in the Control condition ($F_{1,101} = 12.93, p < .001$). The means in Table 9 clearly do not support a hypothesis of reduced blocking in the High Prior Beliefs condition. The main effect of Prior Beliefs was not statistically significant ($F < 1$).[1] A similar ANOVA for brand name yielded a significantly higher Brand premium in the Experimental cells than in the Control cells ($F_{1,101} = 15.34, p < .001$). This effect was not qualified by significant higher-order interactions. Brand premiums were higher in the Low Prior Beliefs condition than in the High Prior Beliefs condition ($F_{1,101} = 7.09, p < .01$). Thus, the results for the willingness-to-pay measure showed significant effects of pre-exposure on the premiums subjects placed on the brand name and the level of the critical attribute that had been paired with a good performance in the learning phases. In addition, subjects showed a lower brand premium when specific prior beliefs favored the critical attribute.

Experiment 2A

The blocking effect was described as the failure of the critical attribute to acquire value due to previous learning of brand name-quality associations. Thus, the assumption was made that subjects learned about the brand-quality relationship in the first learning phase. A process check was performed by assessing the value subjects place on brand names after the first learning phase. In addition to providing a process check, measuring

---

[1] When the same analysis was done on the untransformed data, the blocking (pre-exposure) main effect was still significant ($F_{1,101} = 8.10, p < .01$), but the main effect was qualified by a Prior Beliefs by pre-exposure interaction ($F_{1,101} = 9.88, p < .01$) indicating that blocking effect was stronger when the subjects had strong specific prior beliefs than when they had weaker specific prior beliefs. In fact, the analysis on the raw data suggests that the blocking effect is totally absent when the subjects do not have strong prior beliefs. This finding goes contrary to all other measures in both Experiments 1 and 2. For the Brand premium, statistically significant effects were the same for raw and normalized data. See Appendix D for

the influence of brands on product evaluations after Phase 1 provided a baseline for assessing the extent to which exposure to critical attribute information after brand pre-exposure leads to a decrease in brand equity.

Method. Twenty-three subjects participated in this experiment. The procedure and stimuli in Experiment 2A were identical to those in the Low Prior Beliefs Experimental conditions in Experiment 2 with the exception that the second learning phase was omitted. To make the results comparable to the results in Experiment 2, the answer booklet in Experiment 2A contained the same eight quality judgment items as in Experiment 2. In addition, four profiles giving information about only three features (brand, warranty, and country of origin) were added to check for inconsistencies due to the fact that the first eight profiles contained information about a feature (rudder) that the subjects had not encountered before. The four extra profiles were constructed as a 2 * 2 factorial combination of the two levels of the brand and the uncorrelated filler feature. The willingness-to-pay measure was omitted in Experiment 2A.

TABLE 10
EXPERIMENTS 2 AND 2A: FREQUENCIES OF SUBJECTS SHOWING QUALITY JUDGMENT PATTERNS

	Frequencies (Proportions)	
Judgments Depending On:	After Phase 1	After Phase 2
Brand	17 (.74)	24 (.73)
Other	6 (.26)	9 (.27)

Results. Frequencies of quality judgment patterns after the first learning phase and comparable data after the second phase (from Experiment 2) are presented in Table 10. Results showed that a very high proportion of subjects (74%) judged all Hypalon brand

---

raw data means.

rafts to be high quality and all Riken brand rafts to be low quality. This result indicated that the majority of subjects had indeed learned the relationship between brand and quality in just four trials despite the presence of two filler features. In addition, a comparison of the classification patterns before the presentation of critical attribute information (Experiment 2A) and after a large number of experiences (12 or 24 trials) with critical attribute information (Low Prior Beliefs-Experimental conditions in Experiment 2) yields very similar proportions of brand-driven patterns of quality judgments (74% before and 73% after receiving critical attribute information). Thus, in this case, no evidence was found for any decrease in brand equity whatsoever when subjects were exposed to critical attribute information. These data are also consistent with the assertion that blocking obtained for all subjects who initially learned the brand-quality relationship and that any acquisition of value by the critical attribute in the second learning phase by subjects in the Experimental condition was due to a lack of learning in Phase 1. This suggests that the results reported in the preceding experiments were very conservative.

The frequencies of subjects showing brand-driven quality judgments in the four-item task were almost identical to those in the eight-item task. In this task, 19 (83%) of the subjects showed a brand-driven pattern, while four (17%) showed another pattern.

Discussion

The goal of Experiments 1, 2, and 2A was to determine whether brand equity can be preserved in the face of intrinsic attribute information. Experiments 1 and 2 clearly showed that brands can preserve a large amount of equity when consumers are exposed to brand and performance information before they receive information about critical

attributes. Indeed, the comparison of results from Experiments 2 and 2A suggest that the preservation of brand equity can be perfect. In other words, these results suggest that providing consumers with diagnostic critical attribute information does not necessarily lead to a decrease in brand equity and an increase in the weight consumers put on critical attributes. In contrast to the pre-exposure case, the results suggest that when consumers learn about brands and attributes simultaneously, a decrease in brand equity will occur and consumers will put a high weight on critical attribute information. Pre-exposing subjects to brand information leads to increased differences in product evaluations and willingness-to-pay for products with different brands but identical critical attributes and to reduced difference in product evaluations and reduced price premiums for products with different levels of the critical attributes.

Experiments 1 and 2 also investigated the influence of two types of causal beliefs on the blocking phenomenon. The results suggest that although consumers seem to have these beliefs and that they influence their product evaluations, they may not reduce the effects of pre-exposure. Although no significant interactions were found between the beliefs manipulations and the effects of pre-exposure, results in Experiment 2 do suggest that the weight that consumers put on brands (critical attributes) can be decreased (increased) when consumers have strong specific priors favoring the importance of specific critical attributes for specific outcomes relative to situations in which consumers do not have such priors. In addition to the robustness of the pre-exposure effects (blocking and preservation of the predictive value of the first predictive feature) to prior beliefs favoring the critical attribute, the results in Experiments 1 and 2 also showed that these effects persisted in the face of (i) a very low number of pre-exposures (four), (ii) a

relatively large number of exposures to the blocked feature (24), (iii) the absence of explicit trial-by-trial prediction and feedback, and (iv) the blocked feature being the only intrinsic attribute in the task.

CHAPTER 5
EXPERIMENT 3

Experiments 1 and 2 yielded strong and robust effects of exposure to brand information prior to exposure to critical attribute information. These blocking and "brand equity preserving" effects appeared despite the fact that Experiments 1 and 2 were designed to afford subjects every opportunity to learn to value the critical attribute. To the best of my knowledge, Experiments 1 and 2 constituted a much more conservative set of demonstrations of the blocking phenomenon than any other previous demonstration. The fact that those very conservative experiments yielded robust effects suggests that the blocking and brand equity preserving effects will generalize to many situations that structurally fit the classical A+ -> AB+ paradigm. Although these situations are probably very common in consumer contexts, there are also many situations that are structurally different. For example, whereas many initial exposures to product information include only information about brand names and consumption outcomes, advertising for some products consistently includes information about a "positioning attribute" that accompanies the brand name. In many cases, this positioning attribute appears valuable, but is actually irrelevant to product performance (Carpenter, Glazer, and Nakamoto 1994). Findings by Carpenter, Glazer, and Nakamoto (1994) suggest that including such an "irrelevant attribute"[1] would increase the evaluations of products that possess it. It is

---

[1] The term "irrelevant attribute" was borrowed from Carpenter et al. (1994). The analysis applies equally to "relevant attributes" that do have a direct influence on product performance, because subjects in the

unclear, however, what exposure to an extra attribute would do to blocking of a critical attribute and to the effect of pre-exposure on brand equity. For example, adding an irrelevant attribute might increase the blocking effect because the critical attribute becomes even more redundant when consumers learn about two perfect predictors (brand name plus the irrelevant attribute) instead of one (brand name) before they are exposed to critical attribute information. It is also possible, however, that adding the irrelevant attribute will decrease the blocking effect when neither of the two first phase predictive features (brand name and the irrelevant attribute) are learned sufficiently well to block the third predictive feature (the critical attribute). Experiment 3 was designed to determine the effect of adding an irrelevant attribute to a brand name on the blocking of a critical attribute. In addition to assessing the effect of adding irrelevant attributes on the value of critical attributes, Experiment 3 also was designed to assess the impact of adding irrelevant attributes on the value that consumers place on brand names. For example, several theories of animal and human learning (e.g., Rescorla and Wagner 1972) and of human causal induction (e.g., Kelley 1973; Cheng 1997) predict that the brand name will "compete" for equity with the irrelevant attribute and is likely to loose at least some of its equity relative to the situation in which no irrelevant attribute is used. This competition should reduce the brand equity enhancing effect of brand name pre-exposure. The loss in brand equity due to competition with the irrelevant attribute should hurt consumer

---

experiment were never told that the added attribute was "irrelevant" and there is no reason to assume that they would regard it as less relevant than any other attribute. The discussion focuses on "irrelevant attributes" to demonstrate, like the earlier experiments, how pre-exposure to non-causal information can affect consumers' learning of the attribute(s) that actually drive product performance. In addition, the types of "irrelevant" positioning attributes described by Carpenter et al. (1994) should be less easily copied than standard intrinsic attributes or ingredients. For example, a competitor copying the "Alpine Class" attribute in down jackets would be more likely to suffer from consumer resentment against copycats than a competitor copying a fill rating, something that each jacket is expected to have. Note that companies should have little incentive to position their products using easily copied attributes.

evaluations of brand extensions in cases in which the irrelevant attribute cannot be used in the extension category.

TABLE 11
EXPERIMENT 3: DESIGN SUMMARY

Irrelevant Attribute / Experimental vs. Control	Experimental Phase: Learning 1	Learning 2	Test
No Irrelevant Attribute			
Experimental	$A_{br1}+$ $A_{br2}-$	$A_{br1}B_{ca1}+$ $A_{br2}B_{ca2}-$	$A_{br}?$ $A_{ia}?$ $B_{ca}?$
Control		$A_{br1}B_{ca1}+$ $A_{br2}B_{ca2}-$	$A_{br}?$ $A_{ia}?$ $B_{ca}?$
Irrelevant Attribute			
Experimental	$A_{br1}A_{ia1}+$ $A_{br2}A_{ia2}-$	$A_{br1}A_{ia1}B_{ca1}+$ $A_{br2}B_{ca2}A_{ia2}-$	$A_{br}?$ $A_{ia}?$ $B_{ca}?$
Control		$A_{br1}A_{ia1}B_{ca1}+$ $A_{br2}B_{ca2}A_{ia2}-$	$A_{br}?$ $A_{ia}?$ $B_{ca}?$

Note: Abbreviations used are "br" for "brand," "ia" for "irrelevant attribute," "ca" for "critical attribute," "+" for "high quality," and "-" for "low quality." Subscripts "1" and "2" refer to different levels of a feature.

Method

One-hundred-sixty subjects participated in small groups of up to nine persons. The procedure and format of the stimuli were similar to those used in the earlier experiments. However, a different, more familiar product category (down jackets) was chosen with features taken from the research by Carpenter et al. (1994; see Appendix B). For all subjects, the first predictive feature was brand name, again with levels Hypalon and Riken. Hypalon was always coupled with high quality and Riken was always coupled with low quality. The filler features were "extra tight stitching" (constant) and "cover

material," which was either "cotton" or "synthetic" (uncorrelated with the outcome). In all cases, the critical attribute was fill rating, which had the "550" level for the high quality jackets and the "500" level for the low quality jackets. Finally, half the subjects received information about an irrelevant attribute in addition to the brand name. All high quality products had "alpine class down fill," and all low quality products had "regular down fill." The first learning phase consisted of four learning trials. The second learning phase consisted of 12 learning trials. In sum, the basic design of Experiment 3, summarized in Table 11, was a 2 (Experimental vs. Control) * 2 (Brand-Only versus Brand-Plus-Irrelevant Attribute) completely randomized factorial design.

Three dependent measures were used. Two measures each consisted of 16 down jacket profiles, constructed as a 2 * 2 * 2 * 2 factorial combination of the two levels of the brand name, the irrelevant attribute, the uncorrelated filler feature, and the critical attribute. For the first measure, subjects indicated whether each profiled product was high or low quality. The second measure was identical but employed seven-point scales ranging from -3 (low quality) to +3 (high quality).[2] A third measure consisted of two profiles with seven-point scales in a different product category: woolen sweaters. Both profiles mentioned that the wool used in the product originated in Ireland, that the product was machine washable, and that the type of wool used was lambswool. The profiles in this "extension category" only differed in terms of the sweater's brand name: Hypalon versus Riken. Thus, the third measure allowed measurement of brand equity in an extension category.

---

[2] Because the two 16 profile measures were very similar, the order of the two types of profile measures was manipulated in a pretest to test for effects of making binary quality judgments first on later quality judgments using seven-point scales. The pretest yielded no significant order effects and significant pre-exposure effects were found in both order conditions.

TABLE 12
EXPERIMENT 3: MEAN FEATURE EFFECTS ON BINARY QUALITY JUDGMENTS (DOWN JACKETS)

	Mean Feature Effects		
Irrelevant Attribute / Feature	Control	Experimental	Difference
No Irrelevant Attribute			
Brand (A)	.34	.68	.34
Fill Type (IA)	.02	.00	-.02
Fill Rating (B)	.56	.27	-.29
Irrelevant Attribute			
Brand (A)	.24	.23	-.01
Fill Type (IA)	.29	.60	.31
Fill Rating (B)	.51	.19	-.32

Results

Blocking. Results from the two measures in the original category, down jackets, showed a strong blocking effect that was not significantly affected by the addition of the irrelevant attribute. In other words, there was no evidence to suggest that the effect of pre-exposure on the predictive value of the critical attribute (fill rating) was stronger or weaker when pre-exposure involved brand name plus an irrelevant attribute (fill type) instead of brand name alone.

The results for the binary quality judgment measure were analyzed using three logistic regression models, each including the levels of one of the three predictive features and of the two between-subjects independent variables plus all the simple and higher-order interactions between the predictive feature and the between-subjects variables. Table 12 summarizes the descriptive results for the 16-profile measure with binary quality judgments.

As in Experiments 1 and 2, just four pre-exposures to predictive information before receiving information about another predictive feature, the critical attribute, significantly decreased the effect of that critical attribute (fill rating) on binary quality judgments relative to a situation in which all predictive features were presented simultaneously (asymptotic $t = -5.09$, $p < .0001$). This blocking effect was not significantly dependent on whether the former predictive information included information about an irrelevant attribute (asymptotic $t = .50$, n.s.). There was, however, a small but statistically significant overall effect of adding an irrelevant attribute on the mean feature effect of the critical attribute (asymptotic $t = -2.79$, $p < .01$). This effect indicated that the inclusion of another predictive attribute decreased the effect of the critical attribute, regardless of pre-exposure. The latter finding is consistent with the idea of competition between cues and suggests that adding an irrelevant attribute may decrease the weight consumers place on critical attributes and, hence, decrease competition from competing products that share the same critical attributes.

The results for the 16-profile measure with seven-point quality judgments were analyzed using an omnibus ANOVA (see Table 13 for a summary of means). One subject failed to complete this measure. Thus, the results for the 16 seven-point quality judgments were based on data for 159 subjects. Consistent with the findings for the binary quality judgments, a significant overall blocking effect was found ($F_{1,155} = 21.25$, p <.001). This effect did not significantly depend on the presence or absence of an irrelevant attribute ($F_{1,155} < 1$). The blocking effect was qualified by two higher-order interactions. The first indicated that the effect of pre-exposure on the weight of the critical attribute was stronger (*MFE* = -1.67) for Hypalon brand profiles than for Riken

brand profiles (MFE = -1.17; $F_{1,155} = 8.77, p < .01$). However, even for the Riken profiles, the blocking effect was statistically significant; $F_{1,155} = 14.24, p < .001$). This interaction with brand name was in turn qualified by a significant interaction with the levels of the irrelevant attribute ($F_{1,155} = 6.13, p < .05$). This four-way interaction effect indicated that the first qualifying effect of brand name was stronger for the profiled products with regular down fill than for those with alpine class down fill. While influencing its size, these qualifying effects never eliminated the blocking effect. Even for the Riken brand-regular down fill profiles, a statistically significant blocking effect was found (MFE = -.97; $F_{1,155} = 9.20, p < .01$). Unlike the binary quality judgments measure, results for the seven-point scale measure did not show a significant overall decrease of the weight of the critical attribute (fill rating) in cells with irrelevant attribute information (fill type) relative to those that did not include information about an irrelevant attribute.

TABLE 13
EXPERIMENT 3: MEAN FEATURE EFFECTS ON SEVEN-POINT QUALITY JUDGMENTS (DOWN JACKETS)

	Mean Feature Effects		
Irrelevant Attribute / Feature	Control	Experimental	Difference
No Irrelevant Attribute			
Brand (A)	1.89	3.25	1.36
Fill Type (IA)	.32	.24	-.08
Fill Rating (B)	2.81	1.41	-1.39
Irrelevant Attribute			
Brand (A)	1.28	.90	-.38
Fill Type (IA)	1.41	2.61	1.20
Fill Rating (B)	2.34	1.16	-1.18

Brand and irrelevant attribute. As in Experiments 1 and 2, the 16-profile measure with binary quality judgments (see Table 12) showed a brand equity enhancing effect of

pre-exposure (asymptotic $t = 6.32, p < .0001$). However, this effect was qualified by a significant interaction with whether or not brands were accompanied by information about an irrelevant attribute (asymptotic $t = -4.72, p < .0001$). In addition, an overall negative effect was found of the inclusion of an irrelevant attribute on the effect of brand names on quality judgments (asymptotic $t = -7.55, p < .0001$). The interaction between pre-exposure and inclusion of an irrelevant attribute prompted further analyses for the cells with and without irrelevant attribute information separately.

When no irrelevant attribute was used in the stimuli, the same type of brand equity preserving effect of pre-exposure was found as in Experiments 1 and 2 (asymptotic $t = 6.32, p < .0001$). Subjects put much more value on brand names when they were pre-exposed to brand information (*MFE* = .68) than when brand and critical attribute information were presented simultaneously (*MFE* = .34). However, when brands were accompanied by information about an irrelevant attribute, the brand equity preserving effect of pre-exposure disappeared (asymptotic $t = .06$, n.s.). The effect of brand name on quality judgments became very small in both the Experimental (pre-exposure; *MFE* = .24) and Control (simultaneous exposure; *MFE* = .23) conditions. The total absence of a significant brand equity enhancing effect of pre-exposure and the overall small size of the brand effect on product evaluations in the irrelevant attribute conditions suggests that the irrelevant attribute "overshadowed" (e.g., McSweeney and Bierley 1984; Pavlov 1927) or "outcompeted" the brand name, severely decreasing brand equity in Control and Experimental cells.

Results for the irrelevant attribute suggested that the irrelevant attribute had "taken over" the blocking role normally performed by brands in the standard paradigm. An

analysis for the two cells in which the subjects had been presented with irrelevant attribute information during learning, showed that the difference in quality classifications between alpine class down fill jackets and regular down fill jackets was much larger when consumers learned about brand name and irrelevant attribute before they received information about the critical attribute ($MFE = .60$) than when they received information about all three predictive features simultaneously ($MFE = .29$; asymptotic $t = 6.05$, $p < .0001$).

For brand equity, the results for the 16-profile measure with seven-point quality judgments (see Table 13) were again fully consistent with the findings for the binary quality judgments. The overall effect of brand name on quality judgments declined significantly when an irrelevant attribute was included ($F_{1,155} = 24.11$, $p <.0001$). The effect of pre-exposure on the effect of brand name on quality judgments was not statistically significant overall ($F_{1,155} = 2.66$, $p = .10$). This effect was contingent on the presence versus absence of irrelevant attribute information in the stimuli ($F_{1,155} = 8.41$, $p <.01$). Whereas pre-exposure lead to a significant increase in the effect of brands on quality judgments when no irrelevant attribute information was presented ($F_{1,77} = 7.95$, $p <.01$), such an increase was absent when the brands were accompanied by an irrelevant attribute ($F_{1,78} = 1.13$, n.s.). This three-way interaction between levels of Brand, pre-exposure, and the presence or absence of irrelevant attribute information was not qualified by any significant higher-order interactions (*p*s >.05).

The effect of the irrelevant attribute on product evaluations was, of course, dependent on whether or not an irrelevant attribute was ever presented in the stimuli ($F_{1,155} = 94.52$, $p <.001$). This effect was in turn moderated by an effect of pre-exposure

versus simultaneous exposure ($F_{1,155} = 12.91, p <.001$). In the cells in which no irrelevant attribute was ever presented, the effect of the irrelevant attribute was very small (*MFE* = .28; but statistically significant, $F_{1,77} = 17.14, p <.001$) and did not significantly depend on pre-exposure versus simultaneous exposure of the brand name ($F < 1$). In the cells in which an irrelevant attribute had been presented in the learning trials, its effect was significantly higher in the Experimental (pre-exposure) condition (*MFE* = 2.61) than in the Control (simultaneous exposure) condition (*MFE* = 1.41; $F_{1,78} = 13.43, p <.001$).

TABLE 14
EXPERIMENT 3: MEAN BRAND EFFECT ON SEVEN-POINT QUALITY JUDGMENTS (WOOLEN SWEATERS)

	Mean Feature Effects[a]		
Irrelevant Attribute	Control	Experimental	Difference
No Irrelevant Attribute	2.55	3.38	.83
Irrelevant Attribute	1.40	1.43	.03

The "overshadowing" effect of the use of an irrelevant attribute on brand equity was replicated in the extension category measure (see Table 14). The brand effect on evaluations in the extension category decreased significantly when an irrelevant attribute had been used in the original product category ($F_{1,153} = 13.95, p <.001$).[3] The brand equity enhancing effect of pre-exposure in the cells in which no irrelevant attribute information was included, was directionally consistent with the results for the other measures, but was not statistically significant ($F_{1,75} = 1.59$, p < .22).

Discussion

Experiment 3 showed that the blocking effect generalized to a situation in which initial exposure was to a brand name plus an "irrelevant attribute." An equally strong

[3] Four subjects failed to complete this measure.

blocking effect of pre-exposure on the value to consumers of a critical attribute was found when consumers were exposed to a brand name plus an irrelevant attribute as when they were exposed to a brand name-only and only the binary quality judgments measure showed a (small) significant main effect of the inclusion of an irrelevant attribute on the predictive value of the critical attribute. However, adding an irrelevant attribute did have a very important impact on the value consumers placed on brand names (i.e., on brand equity). Both in the original product category and in an extension category, brand names had much less impact on product quality judgments when they had been coupled with an irrelevant attribute in the original category than when they had never been coupled with an irrelevant attribute. In fact, the brand equity enhancing effect of pre-exposure to a brand name totally disappeared when it had been coupled with an irrelevant attribute. In sum, Experiment 3 suggested that blocking of a predictive attribute should generalize to situations in which brands are accompanied by positioning attributes and that brand equity may be severely damaged by the use of salient positioning attributes. On a more general level, Experiment 3 demonstrated that product features "compete" for predictive value, with important consequences for brand and attribute equity.

The descriptions of Experiments 1, 2, and 3 focused mainly on potential moderators of the blocking phenomenon and did not address the issue of the process underlying the blocking effect. The remaining Experiments in this dissertation were designed mainly to explore different explanations of the blocking phenomenon (and its brand equity preserving counterpart). The first of those accounts, the "encoding account" (e.g., Price and Yates 1995) was examined in Experiment 4.

CHAPTER 6
EXPERIMENT 4

The blocking effect and some other effects in the animal and human literatures on predictive and causal learning have been attributed to an attentional effect, although few authors in the past 25 years have subscribed to this account or even mentioned it. This "encoding account" (e.g., Price and Yates 1995) holds that subjects learn quickly which cue is most predictive of an outcome and then direct most or all of their attention to that cue. According to this explanation, subjects in the first phase of a blocking design quickly learn that the first cue (A) perfectly predicts the outcome (+) and, once they have learned that A perfectly predicts the outcome, stop paying attention to other stimuli. Such a lack of attention to other stimuli, including the B cue, would make subjects unable to remember the stimulus instances and compute the co-occurrence of the outcome (e.g., quality) and the B cue (e.g, a critical attribute) at a later time.

Truncated Search and Attention in Predictive and Causal Learning

Given humans' and animals' attentional deficits, this behavior seems efficient, perhaps even normative. In a series of studies on information search in causal analysis, Shaklee and Fischhoff (1982) found that when subjects were asked to explain an event with multiple possible causes, they searched for information about one cause. Once enough support had been gathered to assume the sufficiency of that one cause (i.e., to assume that

P[Event|Cause Present] = 1), they did not proceed to consider other potential causes. Thus, search was truncated as soon as one predictive cue for the outcome had been found.

The learning situations in most human blocking studies seem to make truncated search, or truncated attention, an unlikely candidate to explain the strong blocking effects found for several reasons. First, unlike in Shaklee and Fischhoff's studies, subjects in a blocking experiment are actively exposed to the information about the second predictive cue. They do not have to search for it. Second, the number of trials in the second learning phase, and hence the number of exposures to the second cue, is usually quite high (e.g., 24 exposures in experiments by Chapman and Robbins, 1990; 48 exposures in experiments by Waldmann and Holyoak, 1992). Third, the two learning phases in a blocking design are often separated by a short break (often used to measure learning in the first phase; e.g., Chapman 1991; Chapman and Robbins 1990; Waldmann and Holyoak 1992) and subjects are usually instructed to pay attention again after the break. Fourth, subjects are often explicitly instructed to pay attention to all the cues and are explicitly told that they will be asked to rate the predictiveness of each cue separately (e.g., Chapman 1991; Chapman and Robbins 1990; Waldmann and Holyoak 1992). Finally, in some blocking studies (e.g., Chapman and Robbins 1990; Waldmann and Holyoak 1992), subjects rate the predictiveness of the B cue after half the trials in the second learning phase. This should sensitize them to attend to the B cue in the remaining trials, leading to increased ratings in the final rating task after the conclusion of the second learning phase. However, declines in blocking between the first and second set of measures during and after the second learning phase have not been found.

In addition to these "practical" reasons, there is also empirical and theoretical evidence against an attentional explanation in both the animal and human literatures (see, e.g., Kamin 1969; Rescorla and Wagner 1972 for evidence in the animal learning literature). First, an explanation in terms of truncated attention is not parsimoneous in the sense that it cannot account for most other phenomena in the animal and human causal and predictive learning literature that can be explained by the associative and contingency accounts. For example, Rescorla and Wagner (1972, p. 95) explained how an attentional explanation cannot account for unblocking (e.g., Shanks 1991; Wagner 1969) and conditioned inhibition (e.g., Chapman and Robbins 1990; Pavlov 1927). Second, Price and Yates (1995) found empirical evidence against an attentional explanation. In several experiments revolving around the "relative validity effect" (see, e.g., Baker, Mercier, Vallée-Tourangeau, Frank, and Pan 1993; Wagner, Logan, Haberlandt, and Price 1968), Price and Yates presented subjects with information about a disease (outcome) and two potential causes. For the "common" cause, the disease was somewhat more likely in its presence than in its absence. For the "context" cause, the disease was either equally likely in the absence of the context cause as in its presence (weak-context condition) or much more likely in the context cause's presence than in its absence (strong-context condition). When subjects were asked to rate how certain they were that the common cause caused the disease, they were more certain in the weak-context condition than in the strong-context condition. The same result obtained when they were asked to indicate the predictiveness of the common cause for the disease (i.e., P[Outcome|Common Cause]). However, no such difference was found when subjects were asked to indicate the four frequencies that form the 2 * 2 contingency table between the common cause

(present-absent) and the outcome (present-absent). In fact, subjects gave very accurate frequencies for all four frequencies in both conditions. Thus, their results were inconsistent with the encoding deficit account.

Evidence for Truncated Attention as an Explanation of Blocking

Notwithstanding the evidence and arguments presented against the attentional account, it may be that subjects cannot accurately assess the predictive relationship between the second cue (B) and the outcome due to lack of attention in situations in which learning of the first cue (A) in the first learning phase is strong, the number of second phase (AB+) trials is limited, and second phase stimuli are complex because of the inclusion of a large number of filler cues not present in the first phase. Support for such an explanation was found by Sanbonmatsu et al. (1994). In a series of experiments, subjects were given information about eight students taking a course in welding. This information included whether the student passed or failed the class (outcome) as well as their gender (A; male or female), course load (B; 10, 11, 15, or 16 hours), marital status, age, grade point average, hobbies, employment status, church membership, socioeconomic class background, and birth order (filler cues). In these eight profiles, gender and class load were predictive of course success, whereas the filler cues were uncorrelated with course success. For each student profile, subjects read cue information, predicted whether the student would be successful in the course, and received feedback. After receiving the information about all eight students, subjects estimated the frequencies of successful and unsuccessful students who had a high versus low course load, i.e., they estimated the frequencies of cues given outcomes. In addition, subjects

were asked to indicate the relative causal influence of the different cues on course success.

Although this basic design (AB+) is quite different from the standard blocking design (A+ -> AB+), this experiment may speak to the same phenomenon. According to Sanbonmatsu et al. (1994), subjects had very strong prior beliefs about the relationship between the specific levels of the gender variable and the outcome. That is, they believed that male students should do well and that female students should not do well. This type of prior belief may function exactly like the first phase in a blocking design.[1] Thus, a case can be made for the structural equivalence of this design with a traditional blocking design. The results indicated that subjects could not correctly remember the frequencies for the "blocked variable" (B; course load) and that they perceived it to have a minor role in causing the outcome (course success). Follow-up analyses suggested that the causal judgments were partly mediated by the frequency judgments.

Experiment 2 and Truncated Attention

In Experiments 1, 2, and 3, learning of the first predictive feature (A) in Phase 1 was probably less strong than the extra-experimental beliefs about gender and success in a welding course that are the result of years of socialization. In addition, the number of new filler features in Phase 2 in Experiments 1, 2, and 3, was zero and the overall number of filler features was just two, one of which, the constant cue, had just one level. Thus, the attentional account seems less likely as a candidate in the experiments presented here than in the experiments by Sanbonmatsu et al. (1994). In addition, the number of trials in

[1] This type of prior beliefs is different from the type of prior beliefs manipulated in Experiment 2. In Experiment 2 subjects probably knew that a cue dimension (e.g., gender, rudder) should be important for an outcome (e.g., success in a welding course, steering performance), but did not know which level of the cue

Phase 2 was manipulated in Experiment 2. If subjects learned quickly that the first predictive feature (e.g., brand) was perfectly predictive of the consumption outcome and then directed most but not all of their attention to that first predictive feature, one would expect that blocking would still have been reduced in the Experimental cells of Experiment 2 when the number of exposures to the critical attribute in Phase 2 was doubled from 12 to 24. In other words, unless attention to other cues was "blocked" 100 percent, each extra trial should still have made subjects in the pre-exposure conditions more likely to identify the predictiveness of the critical attribute. Thus, the predictive value of the critical attribute should have been higher in the Experimental cells with 24 trials than in those with 12 trials. The results in Experiment 2 did not support this prediction. However, these results could still be explained by a strong version of the encoding account. Such a "strong" encoding hypothesis would hold that as soon as one predictive cue is identified, (almost) no attention whatsoever is paid to other cues, regardless of the number of exposures to those other cues. In sum, the results of the manipulation of the number of Phase 2 trials in Experiment 2 were inconsistent with a "weak" encoding account, which would have brought sizeable differences in the mean feature effects of the critical attribute between the Experimental cells with 12 trials and those with 24 trials in Phase 2, but they do not discredit a "strong" encoding account which would prevent encoding regardless of the number of Phase 2 trials.

Overview of Experiment

Experiment 4 addressed the encoding account by asking subjects in a standard two-phase blocking condition to recall the pattern of co-occurrence of the critical attribute and

---

(male or female, fourcase or backbar) should lead to which level of the outcome (pass or fail, good or bad).

the outcome in addition to making binary quality judgments. Consistent with the findings by Sanbonmatsu et al. (1994), the encoding account predicts a negative relation between blocking and correct perception of the pattern of co-occurrence between levels of the outcome and levels of the critical attribute. Thus, subjects who show a "blocked" pattern of quality judgments should be less likely to correctly determine the proportions of high (low) quality products that had certain levels of the critical attribute than subjects who did not show a "blocked" pattern of quality judgments. In addition, a negative correlation would be expected between subjects' ability to recall the pattern of occurrence between brand names and quality and their ability to recall the pattern of co-occurrence between levels of the critical attribute and quality. Finally, a strong encoding account would predict that subjects who learned the predictive relationship between the first predictive feature (brand) and the outcome, as indicated by their brand-driven quality judgments, should have no significant insight into the pattern of co-occurrence between the critical attribute and the outcome.

Method

Experiment 4 was a two-group experiment in which the perceived degree and direction of co-occurrence in the stimuli between the levels of the critical attribute and quality were assessed in addition to judgments of product quality. In both groups, subjects were pre-exposed to a brand name in the first learning phase before receiving information about a critical attribute in the second learning phase. The only difference between the two groups was that, in the test phase, one group made co-occurrence judgments after making quality judgments whereas the other group made co-occurrence judgments before making quality judgments. This was done to control for order effects.

TABLE 15
EXPERIMENT 4: FOUR VERSIONS OF CO-OCCURRENCE QUESTIONS

Question 1st level, high quality 1st	In the second set of rafts, how many of the HIGH QUALITY rafts had a FOURCASE RUDDER?
	In the second set of rafts, how many of the LOW QUALITY rafts had a FOURCASE RUDDER?
Question 2nd level, high quality 1st	In the second set of rafts, how many of the HIGH QUALITY rafts had a BACKBAR RUDDER?
	In the second set of rafts, how many of the LOW QUALITY rafts had a BACKBAR RUDDER?
Question 1st level, low quality 1st	In the second set of rafts, how many of the LOW QUALITY rafts had a FOURCASE RUDDER?
	In the second set of rafts, how many of the HIGH QUALITY rafts had a FOURCASE RUDDER?
Question 2nd level, low quality 1st	In the second set of rafts, how many of the LOW QUALITY rafts had a BACKBAR RUDDER?
	In the second set of rafts, how many of the HIGH QUALITY rafts had a BACKBAR RUDDER?

Fifty-two subjects participated in Experiment 4. One subject failed to complete the co-occurrence judgments for the critical attribute. Thus, most analyses were based on data from 51 subjects. In this Experiment, the same procedure and stimuli were used as in the 12 Trials-Low Prior Beliefs-Experimental condition from Experiment 2. In Experiment 4, however, the willingness-to-pay measure was replaced by a measure assessing the subjects' judgments of the relationship between the critical attribute and raft quality in the second phase profiles. For half of the subjects, the co-occurrence measure was administered before the quality classification measure. The other subjects filled out the co-occurrence measure after the quality classifications. The basic syntax of the co-occurrence questions was "In the second set of rafts, how many of the (HIGH/LOW) QUALITY rafts had a (LEVEL FEATURE)?." A five-point answer scale with double anchors ranging from 0%: None, all (HIGH/LOW) quality rafts had a (OTHER LEVEL FEATURE) to 100%: All (HIGH/LOW) quality rafts had a (LEVEL FEATURE) in increments of 25%. For half the subjects, the co-occurrence questions mentioned one level of the focal feature. For the other half, the other level was mentioned. Half the

subjects were asked about the high quality rafts first; the other half were asked about the low quality rafts first. All four versions of the two crucial co-occurrence questions are presented in Table 15. In addition to questions about the relation between the critical attribute and quality, questions were also asked about the relations between the other features and quality and between the critical attribute and brand names.

TABLE 16
EXPERIMENT 4: FREQUENCIES OF SUBJECTS SHOWING QUALITY AND CO-OCCURRENCE JUDGMENT PATTERNS

	Frequencies (Proportions)			
Order / Judgments Depending On:	Fully Accurate Co-Occurrence	Partially Accurate Co-Occurrence	Inaccurate Co-Occurrence	All
Quality First				
Brand	9 (.47)	6 (.32)	4 (.21)	19 (1.00)
Other	1 (.14)	1 (.14)	5 (.71)	7 (1.00)[a]
All	10 (.38)	7 (.27)	9 (.35)	26 (1.00)
Co-Occurrence First				
Brand	3 (.25)	4 (.33)	5 (.42)	12 (1.00)
Other	7 (.54)	4 (.31)	2 (.15)	13 (1.00)
All	10 (.40)	8 (.32)	7 (.28)	25 (1.00)

[a]Not all proportions add up to 1.00 due to rounding after the second digit.

Results

Co-occurrence perceptions between the critical attribute and quality were computed by subtracting the perceived percentage of the high quality rafts that had a fourcase rudder from the perceived percentage of the low quality rafts that had a fourcase rudder. Subjects' co-occurrence perceptions were classified into three categories. If subjects correctly identified that 100% of the high quality rafts had a fourcase rudder and that 0% of the low quality rafts had a fourcase rudder or if subjects correctly identified that 0% of the high quality rafts had a backbar rudder and that 100% of the low quality rafts had a backbar rudder, they were classified as "fully accurate." If subjects noted any higher percentage of fourcase rudders for high quality rafts than for low quality rafts or any higher percentage of backbar rudders for low quality rafts than for high quality rafts, they

were classified as "partially accurate." Subjects claiming the rudder in the co-occurrence question was equally often paired with high quality as with low quality were classified as "inaccurate." The same category was used for subjects indicating a lower percentage of fourcase rudders for high quality rafts than for low quality rafts or a lower percentage of backbar rudders for low quality rafts than for high quality rafts.

Results for the experimental cell in which subjects made quality judgments before making co-occurrence judgments did not support an attentional explanation (see Table 16). Of the 19 subjects in this group demonstrating an entirely brand-driven pattern of quality judgments, 15 showed at least partially accurate perceptions of the co-occurrence between the critical attribute and quality. Of those 15, nine were fully accurate, indicating that 100% of the high quality rafts had a fourcase rudder and that 100% of the low quality rafts had a backbar rudder. In fact, logistic regression analyses showed that entirely brand-driven subjects were significantly more likely than not to have an at least partially accurate memory for critical attribute-quality co-occurrence (asymptotic $t = 2.35$, $p < .02$). These results are inconsistent with a strong encoding explanation, which predicts that these subjects would be ignorant of the critical attribute-quality co-occurrence. In addition, the analyses showed that those entirely brand-driven subjects were significantly <u>more</u> likely to show at least partially accurate co-occurrence recall than subjects whose quality judgments were not entirely brand-driven (asymptotic $t = 2.24$, $p < .03$). Finally, a correlation analysis of the critical attribute-quality and brand-quality co-occurrence judgments did not yield a negative correlation between the accuracy of co-occurrence judgments for the brand and the critical attribute ($r = .15$). Thus, these data, like those in Experiment 2, also do not support weaker versions of the encoding account.

The results for the binary quality judgments in this group were similar to those in Experiment 2.

Overall frequencies of co-occurrence judgments for the subjects who made quality judgments after they indicated their perceptions of the co-occurrence between the critical attribute and quality were similar to those for the subjects who made quality judgments first (see Table 16). They again yielded no evidence of a negative correlation between the accuracy of co-occurrence judgments for the brand and the critical attribute ($r = .38$). Thus, results in this group were also inconsistent with the predictions based on the encoding account.

Results for the quality judgments measure did suggest an order effect. More specifically, the results in this group suggested that asking subjects to judge the critical attribute-quality co-occurrence before making quality judgments increased the predictive value they placed on the critical attribute. Whereas 73% (19 out of 26) of the subjects in the group who made quality judgments before co-occurrence judgments showed an entirely Brand-driven pattern of quality judgments (identical to the corresponding percentage found in Experiment 2), only 48% (12 out of 25) of the subjects who were asked to judge co-occurrence first showed an entirely brand-driven pattern of quality judgments. However, a logistic regression analysis showed that this difference did not reach conventional levels of statistical significance (asymptotic $t = 1.81, p < .1$).

Discussion

The results in Experiment 4 did not support the encoding or truncated attention account as an explanation for the blocking phenomenon. The results for a significant number of subjects cannot be explained by their lack of attention to the critical attribute

in the second learning phase. Clearly, many subjects who did not place any value on the critical attribute in their product evaluations had not directed all their attention at the first predictive feature, because they were often perfectly able to recall that the critical attribute co-occurred with quality in the stimuli. In fact, the data suggested that "blocked" subjects had paid more attention to the critical attribute than "non-blocked" subjects and no negative correlations were found between recall for the first predictive feature (brand) and recall for the second predictive feature (the critical attribute: rudder).

Although the effect was not statistically significant, quality judgments were less likely to be brand-driven and more likely to be driven by critical attribute information when subjects made co-occurrence judgments before they made their quality judgments. This result may be due to a "consistency bias." Subjects may have tried to make their judgments for the second measure consistent with their co-occurrence judgments. However, it is also possible that this result is not just a demand effect. It may indicate that forcing consumers to try to recall their experiences makes them use a different process to form their product evaluations, a retrospective and maybe more reasoned, non-associative, process that does not lead to blocking. Whereas the results in Experiment 4 did not support the encoding account, the results were not inconsistent wtih the contingency and associative accounts. Experiment 5 was designed to explore the contingency account.

CHAPTER 7
EXPERIMENT 5

Over the past few years, several authors have tried to explain blocking and other causal and predictive learning phenomena in terms of normative causal models. This group of closely related models has been referred to as "contingency" models (e.g., Wasserman and Miller 1997), "rule-based" models" (e.g., Allan 1993), or "normative" models (e.g., Baker et al. 1996). According to these models, the processes that people use to assess predictiveness and causal impact are similar to those used by scientists. The models essentially hold that people assess causality and predictiveness in two stages that can be compared to the data collection and analysis stages in experimental research. The first stage consists of recording events. In the second stage, (i) records are accessed and coded, (ii) means and crosstabs are computed, and (iii) the coded data are analyzed in ways that can be compared to the execution of an analysis of variance. As with scientists, people try to avoid confounded (sub)sets of data and (sub)sets of data that could return null-effects due to ceiling and floor effects (e.g., Cheng, 1997).

Data Collection and Coding of Potential Signs and Causes

The experiments reported in this dissertation were designed, in part, to speak to the processes of data collection, coding, and analysis. Consistent with the contingency theories, the results from the co-occurrence measure in Experiment 4 indicated that subjects were, in general, rather accurate in the data collection and encoding stage. All

experiments reported so far speak to the issue of the coding of the first predictive feature. Waldmann and Holyoak (1992) showed that blocking disappears when cues are coded as potential effects instead of potential causes. In the same vein, I expected that brands would be encoded as signs or labels and not as potential causes, which would reduce blocking of a second predictive feature, an intrinsic attribute, that should be coded as a potential cause. However, no reduced blocking effect was obtained relative to a situation in which both predictive features were intrinsic attributes. This result suggests either that predictiveness judgments and quality predictions were not based on inferences about causal status or that subjects simply did not interpret brands as signs or labels but rather as direct causes. Although some of the evidence was consistent with the hypothesis of differential coding of brands versus intrinsic attributes, no conclusive evidence was found for such differential coding. The second coding issue, the coding of events in Learning Phase 1 in terms of the critical attribute, was the focus of Experiment 5 and will be discussed in detail after the discussion of the analysis process.

Data Analysis

Like many real-world consumer contexts, the experiments reported thus far were all characterized by confounded independent variables and potential ceiling and floor effects. For example, in the Control conditions, the critical attribute was always perfectly confounded with the first predictive feature; in Experiment 3, none of the predictive features could be assessed independently of all others. In the Experimental conditions, the cue A present-cue B absent and cue A present-cue B present cells allowed at least one unconfounded additional contrast for the critical attribute (B), but the null-effect--the

outcome was always present in both cases--could be due to a ceiling effect because the influence of the A cue (e.g., brand) already brought the effect to ceiling.[1]

In general, contingency models predict that when perfectly unconfounded subsets ("focal sets") of data free of ceiling or floor effects are unavailable for at least some of the cues or "independent variables," people should be uncertain about the status of those cues. This should lead subjects to withold their predictiveness and causality judgments or, when no such option is available, to make judgments with low confidence (e.g., Cheng 1997). The results in the previous experiments were generally inconsistent with this hypothesis. For example, in the predictiveness judgments measure in Experiment 1, subjects were explicitly given the opportunity to indicate the "don't know" answer positioned in the middle of a scale ranging from being certain that a cue was predictive of the outcome to being certain that the cue was not predictive of the outcome. According to Cheng's hypothesis (1997), subjects in both the Control and Experimental conditions should have indicated that they did not know whether the critical attribute (floor) was predictive of the outcome (quality). The results for this measure, however, showed clearly that subjects in the Control groups were certain that the B cue was predictive and that they were significantly more certain than subjects in the Experimental groups. In

[1] It should be noted that the ceiling effect explanation only holds if subjects coded the outcomes in the previous experiments as "additive" (Tversky 1977) features, that is, as outcomes that are either present or absent. For example, in such a situation varying the level of the critical attribute (e.g., floor) conditional on the level of the first predictive feature that had been coupled with high quality (e.g., hypalon brand) would lead to the conclusion that the event "high quality present" always occurred. Hence, the absence of an effect of the critical attribute could be due to a ceiling effect. However, if outcomes were not coded in terms of the presence versus absence of a singular event (e.g., "high quality present" versus "high quality absent") but as an outcome dimension, then the contrast between levels of the critical attribute (e.g., floor) could not be due to a ceiling effect when conditionalized on the level of the first predictive feature that had been coupled with low quality (e.g., riken brand). This would be the case because the outcome in the absence of each level of the critical attribute was low on the outcome dimension, leaving enough "space" to find an excitatory effect. A similar argument can be made for inhibitory effects of the critical attribute at high levels of the outcome dimension.

general, this explanation cannot account for the differences between the Experimental and Control conditions, because subjects should be equally uncertain about the predictiveness or causality of the critical attribute in both types of conditions.

In earlier descriptions of contingency models, some authors included another potential explanation for the blocking effect. This explanation holds that blocking may also appear because people will base their judgments on unconditional contrasts when the appropriate conditional contrasts cannot be computed (e.g., Cheng and Holyoak 1995; Waldmann and Holyoak 1992). However, Experiments 1, 2, and 3 provided little support for this explanation. In the "standard" human blocking paradigm, the number of trials is the same in both learning phases. Hence, it can easily be seen that the unconditional contrasts between presence and absence of the B cue is 1.00 in the Control conditions ($\Delta P = P[E|B] - P[E|{\sim}B] = 1.00 - .00$) and only .75 in the Experimental conditions ($\Delta P = P[E|B] - P[E|{\sim}B] = 1.00 - .25$), assuming, as is usually the case, that the outcome occurs on 50 percent of trials ($P[E|{\sim}B]$ is .50 in Phase 1 and .00 in Phase 2, leading to a combined probability of .25). Thus, if subjects base their judgments on ΔP, a partial blocking effect should be obtained. However, in most experiments presented here, only four pre-exposures were used in Phase 1 versus 12 exposures in Phase 2. Thus, the number of occurrences of the outcome (e.g., high quality) in the absence of the B cue was reduced, leading to a contrast that is not very different from the contrast in the Control conditions. The size of the blocking effect in some situations (e.g., the price measures in Experiment 1 for rafts with new levels of the A cue, which showed a blocking effect that decreased raw dollar premiums for the B cue by a factor three) seems difficult to reconcile with this explanation--unless judgments were extremely sensitive to small

differences in ΔP at high levels of ΔP. If subjects based their judgments wholly or partially on ΔP and were sensitive to small differences in ΔP, then doubling the number of Phase 2 trials from 12 to 24 while keeping the number of trials in Phase 1 constant at just four should significantly increase the weight of the B cue in the Experimental conditions. Such an outcome is expected because the proportion of outcome occurrences in the absence of each level of the B cue should decrease by almost half. Experiment 2 showed no evidence that the mean feature effect of the critical attribute was increased in the Experimental conditions when the number of Phase 2 trials was doubled. The unconditional-contrast hypothesis also has difficulty explaining the results for the irrelevant attribute in Experiment 3. In this experiment, the unconditional contrast for the irrelevant attribute was perfect in both the Experimental and the Control conditions. However, the results indicated a significant difference between the mean feature effects for the irrelevant attribute in the Experimental versus the Control condition. Thus, no evidence was found for the assumption that subjects based their judgments on the unconditional contrasts.

Nothwithstanding the lack of support in Experiments 1, 2, and 3 for both contingency explanations of the blocking effect reported in the literature, one could think of an additional explanation of the blocking effects found here and elsewhere. This additional explanation is based on suggestions by Cheng and Holyoak (1995) that people may base their judgments on a mix of unconditional and available conditional contrasts when the appropriate conditional contrasts cannot be computed. An explanation of blocking along these lines would hold that subjects in the standard blocking scenario rate predictiveness or causality of the B cue around the "don't know" midpoint of the scale because they base

their judgments on a combination of the unconditional contrast and the contrast conditional on the presence of the A cue. The unconditional contrast is (moderately) positive and would lead to a (moderately) positive judgment indicating that the cue was predictive or causal. The conditional contrast is equal to zero and would lead to a negative judgment indicating that the cue was not predictive or causal. Thus, a mix of these contrasts could lead subjects in a blocking scenario to give almost any rating, depending on the weights placed on the two contrasts.[2] The main problem with the suggestion by Cheng and Holyoak (1995) that people use a "mix of contrasts" as their basis for judgment when the appropriate contrasts cannot be computed is that it is virtually unfalsifiable. This is the case because Cheng and Holyoak did not provide a theory that allows one to predict the constitution of the mix in any particular situation.[3] Therefore, Experiment 5 focused on another aspect of the contingency account--data coding.

<u>Coding of Absence and Overview of Experiment</u>

The contingency model explanations of the effects found in the previous experiments all depend on specific assumptions about the retrospective coding of events that took place in the first learning phase. This can be illustrated via Kamin's (1969) classic experiment. In Kamin's experiment, the status of cue B, the light, in Phase 1 was clear. It

---

[2] Note that, on the aggregate level, the same means could be produced if each individual based her judgments on either the unconditional or the conditional contrast, but different individuals used different a different base for judgment.

[3] The same criticism of failing to determine a priori what (conditional) contrasts will be used, applies to contingency models in general--not just in blocking scenarios (e.g., Allan 1993; Shanks 1993a; Shanks, Lopez, Darby, and Dickinson 1996). Other criticisms have focused primarily on contingency models' inability to account for order effects and their inability to account for specific updating patterns of responses over time (see, e.g., Baker et al. 1996; Shanks, Lopez, Darby, and Dickinson 1996; Lopez, Shanks, Almaraz, and Fernandez 1998).

was not on. Also, the status of that cue in Phase 1 was the same as in trials in which the outcome (shock) did not occur in Phase 2. It was not on. Thus, the appropriate conditional contrast could be computed for the A cue (noise) in Phase 1, but not for the B cue (light). The unconditional contrast for the B cue for the two learning phases combined could be computed, because it was clear that the trials on which cue B was not presented meant the same in the two phases. The light was not on. In sum, in an experiment such as Kamin's, it can be assumed that subjects code the first phase trials as "cue B absent;" this coding is normative. However, in the experiments reported here, it is unlikely that subjects would have made such an inference of absence. In Phase 2, floors or fill ratings were not absent or present (i.e., an "additive" feature; Tversky 1977) but, like many product features, had two levels that were not present or absent. They were "substitutive" features (Tversky 1977), that is, features that have several possible levels. It is difficult to imagine how a subject who received information in the second learning phase showing that rafts had either a tubular floor or an i-beam floor would infer that all rafts in the first learning phase must have had no floor at all. Rather, one would expect subjects to be uncertain about what type of floor those rafts must have had. In that case, subjects should become uncertain about the causal status of cue A (e.g., brand), because it is now potentially confounded with cue B. In fact, subjects may have assumed, for example, that the hypalon (riken) brand rafts, which had the tubular (i-beam) floors in Phase 2, also had the tubular (i-beam) floors in Phase 1, where information about floors was missing. In that case, trials in both phases were the same, hence both cue factors were totally confounded in both the Experimental and the Control conditions, and no blocking should be expected by the contingency models.

TABLE 17
EXPERIMENT 5: DESIGN SUMMARY

Experimental vs. Control	Experimental Phase: Learning 1	Learning 2	Test
Experimental	$A_{br1}+$	$A_{br1}B_{ca1}+$	$A_{br}?$
	$A_{br2}-$	$A_{br2}B_{ca2}-$	$B_{ca}?$
Control	$(A_{br1}+)$	$A_{br1}B_{ca1}+$	$A_{br}?$
	$(A_{br2}-)$	$A_{br2}B_{ca2}-$	$B_{ca}?$

Note: Abbreviations used are "br" for "brand," "ca" for "critical attribute," "+" for "high quality," and "-" for "low quality." Subscripts "1" and "2" refer to different levels of a feature.

Although unlikely, it is possible that subjects inferred that the products in Phase 1 did not have any level of the critical attribute (e.g., no floor, no fill rating), that those products all had the same level (e.g. all products in Phase 1 had a third type of floor), or that, for those products, the levels of the critical attribute was varied independently of the brand. In those cases, contingency models could still make the same predictions as in the case in which the B cue is clearly absent in Phase 1. Thus, a strong test of explanations of the effects found in the previous experiments in terms of normative retrospective processing should exclude these inferences as normative possibilities. Therefore, in Experiment 5, a situation was created in which subjects could, normatively, only code the products in Phase 1 with a certain level of the A cue (brand) as having the same level of the B cue (critical attribute) as they had in Phase 2 (see Table 17 for a design summary). In this case, Phases 1 and 2 become essentially the same, and the contingency models would predict that no blocking should be found. In Experiment 5, retrospective coding of the products encountered in Phase 1 was targeted by a very heavy-handed manipulation, which provided subjects with coding information that should turn any retrospective reasoning process (including experimental demand processes) against finding a blocking

effect. Thus, any residual blocking effect would provide evidence against a contingency account of the blocking effect and the other effects found in the previous experiments.

Method

Sixty-eight subjects participated in groups of one to six. Experiment 5 was a standard two-group blocking design in which the Experimental group was pre-exposed to predictive brand information about whitewater rafts before being exposed to predictive Floor information whereas the Control group was exposed to predictive brand and floor information simultaneously. The procedure and stimulus format in Experiment 5 were similar to those in the Experiments 1 through 4. The main difference between Experiment 5 and the earlier experiments lay in the introduction to the dependent measure. Before indicating their quality judgments, subjects in both conditions were given more information about whitewater rafts (see Appendix C for the exact instructions). Embedded in this information were statements that all rafts have floors, that there exist just two types of floors, I-beam floors and tubular floors, and that because whitewater rafts have many different features, most information sources can only provide part of the information. The latter point was illustrated by a sentence saying that two water sports catalogs may be selling exactly the same rafts, but one would provide information about things like country-of-origin whereas the other may go into more detail about technical specs. Then, the following sentence was inserted in the Experimental group but not in the Control group: "In fact, the 12 rafts you saw in the first set of profiles...were exactly the same rafts as the 12 rafts you saw in the second set of profiles..." Finally, subjects were told that despite the fact that they had missing information, they should try to give the most appropriate answer. Thus, these

instructions, in a very heavy-handed way, prevented subjects in the Experimental condition from coding the absence of floor information in Phase 1 as "rafts without floors," "rafts with a third type of floor," or "rafts with random floors." The instructions explicitly told subjects that the rafts in the two phases were the same, implying that all good rafts in Phase 1 actually had a tubular floor and all bad rafts in Phase 1 actually had an i-beam floor. Thus, normative models should predict no blocking whatsoever. Other changes made to accommodate the "different information from different catalogs about the same rafts" story, were (i) the equalization of the number of trials in the two learning phases (12 trials each), (ii) presenting each profile in the first learning phase as coming from "catalog A" and the profiles in the second learning phase as coming from "catalog B," and (iii) the use of a different type of filler features in the two learning phases. In the first learning phase, two extrinsic filler features, country of origin ("made in: Sweden" versus "Canada") and warranty ("1 year"), were used. In the second learning phase, the two filler features were the intrinsic attributes rudder ("fourcase" versus "backbar") and fabric ("polyurethane"). The only dependent measure consisted of eight binary quality judgments for profiles that varied in terms of brand, floor (the critical attribute), and rudder (the uncorrelated filler feature). All eight profiles had polyurethane fabric, and no mention was made of the extrinsic filler features used in the first learning phase. Finally, subjects in the Experimental group filled out a question designed to check whether they had read and attended to the critical part of the instruction saying that the rafts in the two learning phases were the same.

Results

As shown in Table 18, the binary quality judgments showed effects of pre-exposure on the mean feature effects of brand (asymptotic $t = 3.00, p < .01$) and floor (asymptotic $t = -2.51, p < .02$). Of the 34 subjects in the Experimental group, only four did not correctly remember the "same rafts" instruction. Excluding these subjects from the analysis did not change the results (asymptotic $t = 2.99, p < .01$ for brand, and asymptotic $t = -2.56, p < .02$ for floor).

TABLE 18
EXPERIMENT 5: MEAN FEATURE EFFECTS ON BINARY QUALITY JUDGMENTS

	Mean Feature Effects		
Feature	Control	Experimental	Difference
Brand	.41	.63	.22
Attribute	.56	.37	-.19

Discussion

The results from the earlier experiments and present Experiment 5 do not support the contingency account of blocking and other phenomena in the causal and predictive learning literature. In Experiment 5, a significant blocking effect was found despite the fact that subjects were explicitly instructed that the products in the two learning phases were exactly the same. In addition, the blocking effect found here was probably a conservative result given that the cover story explicitly pointed out to subjects that there was much missing information. The realization that information was missing should have caused subjects to be more uncertain about their judgments which, in turn, should have led them to put more equal weights on brand and floor in both conditions, contrary to a blocking effect. Another factor that probably made these findings more conservative

than in the previous experiments was the fact that in the Experimental condition the profiles used for the dependent measure were more similar to the profiles in the second learning phase than in the first learning phase, because the profiles in the test and second learning phases had intrinsic filler features whereas those in the first learning phase had extrinsic filler features. In addition, the fact that the catalog in the first learning phase mentioned only extrinsic features may have made the information in Phase 1 seem less credible than the information from the other catalog in Phase 2, which provided intrinsic attribute information. These facts should all have led subjects to disregard the information in Phase 1 and, hence, should have worked against finding a blocking effect.

The finding of a blocking effect despite this heavy-handed manipulation suggests that retrospective use of normative causal models cannot account for the basic blocking effect found in the experiments reported here. The finding of a blocking effect in Experiment 5 was, however, consistent with the idea that blocking is caused by a forward-looking, predictive associative system that is independent of any retrospective causal reasoning processes. The associative account was further explored in Experiment 6.

CHAPTER 8
EXPERIMENT 6

The results in Experiment 5 and in the previous experiments showed that the effects of pre-exposure could not be explained fully in terms of truncated attention or retrospective reasoning guided by causal models. These findings left associative processes as the one remaining candidate of the process explanations reported in the literature.

Associative Models

During the past decade, a growing amount of evidence has been accumulated suggesting that many complex human behaviors can be explained in terms of simple associative processes that can be described by simple learning models (for reviews, see, e.g., Allan 1993; Shanks 1993b, 1994; Wasserman 1990; Young 1995). According to these models, commonly referred to as "associative" (e.g., Wasserman and Miller 1997), "connectionist" (e.g., Smith 1996; Kruschke 1996), "parallell distributed processing" (e.g., Rumelhart and McClelland 1986), or "neural" or "adaptive network" models (e.g., Gluck and Bower 1988), cues and outcomes are represented by nodes that are linked by connections or associations of a certain strength. When cues are presented in the environment, the input nodes representing those cues will be activated. Depending on the strength of the connections between the activated input nodes and the output nodes representing outcomes, the activation from the input nodes is fed forward to activate

certain output nodes, which, in turn, are translated into an outcome prediction or a behavioral response. Like outcome predictions, associative models hold that judgments of predictiveness and causality depend directly on the activation of specific output nodes upon activation of specific input nodes. Thus, the predictive value of a cue to a consumer depends directly on the strength of the association between that cue and the outcome. Learning takes place when the connection strengths are updated according to a learning rule. These models represent fundamentally predictive, forward-looking systems, that do not include any episodic memory and, hence, are not capable of retrospective processing.

In consumer contexts and in the experiments presented here, more than one cue is often present at the same time. For example, in the second learning phase of the standard blocking design, two predictive cues are presented simultaneously. Two types of associative models have been proposed that represent such configurations of cues in different ways. According to configural associative models, humans and animals represent each configuration as one configural cue. In this view, subjects in a standard A+ -> AB+ blocking situation form two strong associations, one between cue A and the outcome, and one between configural cue AB and the outcome. When subjects are later asked to predict the outcome based on cue B alone, little reaction will be observed because no cue B-only node had been established and, hence, no connection existed between cue B and the outcome. Thus, a blocking effect will obtain. A modern and more complex variant of such a configural theory (Pearce 1994) holds that the presentation of cue B by itself will lead to a partial activation of the AB node leading to limited level of activation of the node(s) representing the outcome. Thus, this theory can account for the partial blocking effects typically found in human blocking experiments.

Both types of configural theories, however, cannot account for the basic differences found between the Experimental and Control conditions in the experiments reported here. In the Control conditions, the B cue was also never presented by itself and the configural AB cue was presented an equal number of times in the Control and Experimental conditions. Hence, these models would all predict equally low feature effects for the B cue (ciritical attribute) in the Control conditions as in the Experimental conditions. This prediction clearly is not supported by the data.[1]

Least Mean Squares

In other associative models, referred to as elemental models, configurations of cues are represented in terms of their constituting elements. Thus, simultaneous presentation of cues A and B leads to the activation of two separate nodes, one representing cue A and one representing cue B. Much empirical support has been found for elemental models in which the updating process of associations between cues and outcomes is guided by a variant of the simple Least Mean Squares (LMS) learning rule (see, e.g., Gluck and Bower 1988; Shanks 1993b, 1994; Shanks, Medin, and Holyoak 1996; Young 1995). Similar formulations have independently emerged in research on classical conditioning in animals (Rescorla and Wagner 1972) and in engineering (Widrow and Hoff 1960) and are commonly referred to as the Rescorla-Wagner (1972), Widrow-Hoff (1960), or delta (Rumelhart and McClelland 1986) learning rule. According to the Least Mean Squares

---

[1] Note that explanations of blocking in terms of exemplar-based memory models (e.g., Kruschke 1992; Nosofsky 1986) can be rejected for the same reasons as configural associative models. Both classes of models represent a complex stimulus as one configural cue instead of an array of constituting elemental cues and make the same predictions regarding the blocking effect.

rule, the change, Δw_{ij}, in the strength of the association from cue i to outcome j on a single trial is given by the following function:

$\Delta w_{ij} = \beta (d_j - o_j)a_i$,

where a_i represents the activation of the input node representing cue i, o_j is the predicted outcome or the activation of the output node representing outcome j, d_j represents a "feedback" input signal representing the actual level of outcome j, and β is a learning rate parameter that can be different for each combination of cue and outcome. Thus, learning according to this rule is error-driven. That is, the direction and amount of updating of the association strength between a cue and an outcome depend on the sign and size of the discrepancy between the predicted outcome and the actual outcome. Importantly, the output, o_j, is an additive combination of the incoming activation received from all activated nodes, or;

$o_j = \Sigma\, w_{ij}a_i$

Thus, the association strength of a cue is limited by the association strengths of other cues present during learning. A cue will not develop a strong association with an outcome if the outcome node is already activated sufficiently by activation from other cue nodes. Translated to consumer contexts, this means that the predictive value of a product feature (e.g., a critical attribute) is limited by the predictive values of other product features (e.g., brand name) and, more specifically, that a feature (e.g., a critical attribute) will not acquire any predictive value when a consumer has already fully learned to predict the outcome based on another, co-present feature (e.g., brand). In sum, the LMS model suggests that product features in general compete for predictive value or equity.

The LMS model accounts for the low predictive value of the B cue in the standard A+ -> AB+ blocking situation because the output (o_j) caused by the activation of the AB compound cue is already close to the desired output (d_j) at the beginning of the second phase due to the strong association between cue A and the outcome that was formed in Phase 1. Because the discrepancy between o_j and d_j is low at the beginning of Phase 2, there is no "room" for the B cue to acquire predictive value. In the AB+ scenario of the control conditions the initial discrepancy is large, giving the B cue more opportunity to acquire association strength. The LMS model can also account for the fact that the brand cue received little predictive value in the control conditions and in the Irrelevant Attribute conditions of Experiment 3 as instances of the "overshadowing" phenomenon (e.g., Pavlov 1927). Presumably, intrinsic attributes such as the critical attribute and the irrelevant attribute had a higher learning rate (β) than brand cues. Thus, the association between an intrinsic attribute and quality gains more strength on each trial than the brand association. As a result, near-zero discrepancies are reached before the less salient brand cue has the opportunity to acquire much association strength. Thus, the intrinsic attributes may have "overshadowed" the brand cues.

The null effect of doubling the number of Phase 2 trials in Experiment 2 was also consistent with an explanation in terms of the LMS model, because after 12 trials (Control conditions), and certainly after 16 trials (Experimental conditions) the discrepancy between predicted and actual outcomes should be minimal, hence, the amount of updating in the additional Phase 2 trials should be small.

In Experiment 5, a blocking effect was found despite the fact that subjects were told that the products in Phase 1 were the same as in Phase 2. This results is consistent with

the LMS model, which predicts that cues that are not presented are not activated regardless of whether these non-presentations represent "absence of information" or "information of absence." According to the LMS model, blocking takes place during learning and is not due to a retrospective process at the time of test.

Experiment 4 indicated that subjects were able to make retrospective judgments about probabilities of cues given outcomes (which are different from more prospective judgments about probabilities of outcomes given cues). Because the LMS model represents a fundamentally forward-looking system, it cannot account for any retrospective judgments. However, the fact that the LMS model does not speak to retrospective judgments does not conflict with the hypothesis that product evaluations and predictiveness judgments are driven by associative processes.

The only result that seems problematic for the LMS model is the effect of the Prior Beliefs manipulation in Experiment 2. Because the actual levels of the rudder critical attribute were not known to subjects before the experiment, the only way the LMS model can account for priors in this experiment is through learning rates. Assuming that the learning rates for the associations between the levels of the rudder feature and steering performance were higher than for the associations between levels of rudder and overall quality, the effect of pre-exposure should have been stronger when the outcome was steering performance than when the outcome was quality. This should have been the case because pre-exposure should have made the discrepancy between predicted and actual outcomes at the beginning of Phase 2 very small. After pre-exposure, any difference in Phase 2 learning rates should have had very little impact because there was "nothing left to learn." The critical attribute should have acquired little predictive value

regardless of its learning rate. However, in the Control conditions, initial discrepancies were high and the extent to which the rudder and brand features "filled" the discrepancy should have been influenced more heavily by their relative learning rates. Thus, the critical attribute (rudder) should have acquired more predictive value in the Control condition when the outcome was steering performance than when the outcome was quality. As a result, the difference between the feature effect of the critical attribute (rudder) in the Experimental versus the Control conditions, i.e., the blocking effect, should have been larger when the outcome was labeled steering performance than when it referred to overall quality. In contrast to this prediction, the results showed no significant difference between these two situations. However, in addition to the fact that null effects in tests without very high statistical power are generally insufficient to discard any theory, the LMS model can account for the null effect if the reasonable assumption is made that the learning rates for the associations between brand names and steering performance were lower than those between brand names and overall quality. In that case, the initial discrepancy at the beginning of Phase 2 should have been lower when the outcome referred to steering performance than when the outcome referred to overall quality. This difference should have reduced the blocking effect when the outcome was steering performance (high prior beliefs condition) and could have compensated for the increased blocking effect due to faster learning of the rudder-steering performance connection in Phase 2, leading to equal blocking for both types of outcomes. Thus, Experiments 1 - 5 support the LMS model's associative explanation of the basic blocking effect, and the LMS model can also account for the influences of all other manipulations. This conclusion extends to a more complex variant of the LMS model, the Attention To

Distinctive Input (ADIT) model proposed by Kruschke (1996), because it is identical to the LMS model when outcome expectations are never disconfirmed and when the outcomes occur equally often. Both conditions were satisfied by all experiments reported here. Another variant of the LMS model, the Revised Rescorla-Wagner Model (Van Hamme and Wasserman 1994), includes decay of association strengths for "expected" but not presented cues. The amount of decay depends on the discrepancy between predicted and actual outcomes and is zero when the discrepancy is zero. This model can also account for the results in the previous experiments in the same way as the standard LMS model.

Prototype Extraction and Overview of Experiment

Experiment 6 was designed to explore the generalizability of the blocking effect and provide a further test of the LMS model and its variants by addressing a property of the LMS model that is different from the blocking and overshadowing phenomena found in the earlier experiments. According to the LMS model, predictive cues that are common to more different cue configurations will acquire a disproportionately strong association with the outcome at the expense of predictive cues that are part of fewer types of configurations. For example, in a consumer learning scenario without pre-exposure, the same number of experiences with a critical attribute (cue B) will lead subjects to place more value on that critical attribute when it is part of two different sets of products with two different brand names (cues A_1 and A_2) than when it coincides with just one brand name (cue A_1). In other words, given an equal number of exposures, a B cue will acquire a stronger association in a A_1B+, A_2B+ scenario with eight trials for each stimulus type than in a A_1B+ scenario with 16 trials for the single stimulus type. This is so because the

association strength between each A cue and the outcome is weaker in the scenario with multiple A cues. This weakness of the association is due to the fact that each A cue is presented less often than when the same A cue is presented on all trials on which a specific outcome occurs. Thus, on any particular trial, the presented A cue provides less output activation in the scenario with multiple A cues than in the scenario with just one A cue. As a result, the total output activation (o_j) on each trial is lower, leading to a larger discrepancy between predicted and actual outcomes, which in turn leads to faster updating of the B cue's association strength on each trial, leaving it with most of the available association strength. This phenomenon is sometimes referred to as the "prototype extraction" property of connectionist models because the features that are most common to stimulus configurations belonging to certain outcome categories acquire the strongest associations with an outcome and therefore will have most impact on outcome classification judgments (e.g., Smith 1996).

In the consumer context, the prototype extraction effect may be referred to as the "competitive commoditization effect," because the presence of additional brands (As) with the same critical attributes (Bs) in a market should result in an increase in the value consumers place on critical attributes at the expense of brand names, even when the number of experiences with critical attributes and their relationship with consumption outcomes are kept constant. This increase in predictive value of the critical attributes at the expense of brand names should lead to more similar evaluations for different brands with the same critical attributes. Thus, the addition of brands with the same critical attributes leads to competitive commoditization because it leads consumers to place more value on critical attributes.

The prototype extraction property of the LMS model is consistent with two "principles" of causal explanation of social events (Van Overwalle 1998). According to the principles or criteria of "simplicity" and "breadth," people tend to prefer explanations that are simpler and account for more data (e.g., Read and Marcus-Newhall 1993; Thagard 1989). To the extent that these principles apply to situations in which consumers try to "explain" product performance based on information about product features, one would expect subjects to place less value on brands and more value on critical attributes as the number of different brands that share a common attribute increases. This should be the case regardless of pre-exposure. However, the LMS model predicts that the effect of the number of brands sharing an attribute declines after pre-exposure to brand names. As long as subjects learn to predict the outcome based on each brand name in Phase 1, leading to zero or small discrepancies at the onset of Phase 2, little or no updating of any kind should happen in Phase 2. Thus, the critical attribute should not acquire much association strength regardless of the number of brands used in Phase 2. Thus, the LMS model predicted that the blocking effect should <u>increase</u> in situations in which several brands share a common attribute relative to a situation in which each brand has its own critical attribute. This hypothesis was tested in Experiment 6 by manipulating pre-exposure (Experimental versus Control) and the number of brand names (two versus four) while keeping the levels of the critical attribute constant at two. For half the subject, each level of the critical attribute was coupled with one brand name, as in the preceding experiments. For the other half of the subjects each level of the critical attribute was common to products with two different brand names (see Table 19 for a design summary).

In addition to providing insight into the issue of process, Experiment 6 also was intended to test the generalizability of the blocking effect to the common case in which consumers have experiences with multiple brands that share their critical attributes.

TABLE 19
EXPERIMENT 6: DESIGN SUMMARY

	Experimental Phase		
# of Brand Pairs	Learning 1	Learning 2	Test
Single Brand Pair			
Experimental	$A_{br1}+$	$A_{br1}B_{ca1}+$	$A_{br}?$
	$A_{br2}-$	$A_{br2}B_{ca2}-$	$B_{ca}?$
Control		$A_{br1}B_{ca1}+$	$A_{br}?$
		$A_{br2}B_{ca2}-$	$B_{ca}?$
Two Brand Pairs			
Experimental	$A_{br1}+$	$A_{br1}B_{ca1}+$	$A_{br}?$
	$A_{br2}-$	$A_{br2}B_{ca2}-$	$B_{ca}?$
	$A_{br3}+$	$A_{br3}B_{ca1}+$	
	$A_{br4}-$	$A_{br4}B_{ca2}-$	
Control		$A_{br1}B_{ca1}+$	$A_{br}?$
		$A_{br2}B_{ca2}-$	$B_{ca}?$
		$A_{br3}B_{ca1}+$	
		$A_{br4}B_{ca2}-$	

Note: Abbreviations used are "br" for "brand," "ca" for "critical attribute," "+" for "high quality," and "-" for "low quality." Subscripts "1," "2," "3," and "4" refer to different levels of a feature.

Method

Eighty-two subjects participated in groups of one to six. The basic design in Experiment 6 was a 2 * 2 completely randomized factorial design. Half the subjects were exposed to the brand and critical attribute simultaneously (Control cells); the other half of the subjects were pre-exposed to brand information (Experimental cells). This factor was crossed with a manipulation of the number of pairs of brand names used (One versus Two Brand Pairs). The stimuli in the Experimental and Control conditions with a single pair of brands were similar to the Low Prior Beliefs conditions Experiment 2. As in

Experiment 2, the first predictive feature was brand ("Hypalon" versus "Riken"), and the same extrinsic filler features were used. The critical attribute in Experiment 6 was floor ("tubular" versus "i-beam"). Phase 1 consisted of eight trials. Phase 2 consisted of 16 trials. The remaining two cells only differed from the One Brand Pair cells in terms of the number of brand names and the number of trials in Phase 1. In these Two Brand Pairs conditions a second pair of brand names ("Leafield" versus "Bering") was added so that each raft carried one of four brand names instead of one of two. Hypalon and Leafield were used to refer to high quality rafts. Riken and Bering were used to refer to low quality rafts. In addition, the number of Phase 1 trials was doubled to 16 in the Two Brand Pairs-Experimental cell.[2] The dependent measure in the One Brand Pair conditions consisted of eight profiles with binary quality judgments constituting a 2 * 2 * 2 factorial combination of brand (Hypalon versus Riken), floor (tubular versus i-beam) and the uncorrelated filler feature ("made in: Sweden" versus "Canada"). In the Two Brand Pairs condition, subjects made the same eight binary quality judgments plus eight additional binary quality judgments created by a 2 * 2 * 2 factorial combination of the second brand pair (Leafield versus Riken), floor, and country-of-origin.

Results and Discussion

The data in Experiment 6 were analyzed using two logistic regressions. The first analysis included the levels of the first brand pair, the between-subjects variables, and all their interactions. The second analysis similarly included the levels of the critical

[2] A pretest was run to make sure that learning of each brand name in Phase 1 was equal in both Brand Pair conditions (One versus Two). This was necessary to equalize the output node activation (o_j) at the beginning of Phase 2 and, thus, prevent alternative explanations in terms of unequal learning in Phase 1.

attribute (floor), the between-subjects variables, and all their interactions. The mean feature effects for brand and floor are presented in Table 20.

TABLE 20
EXPERIMENT 6: MEAN FEATURE EFFECTS ON BINARY QUALITY JUDGMENTS

	Mean Feature Effects		
# of Brand Pairs / Feature	Control	Experimental	Difference
Single Brand Pair			
Brand Pair 1	.30	.69	.39
Attribute	.63	.29	-.34
Two Brand Pairs			
Brand Pair 1	.04	.34	.30
Brand Pairs 1 & 2	.03	.35	.32
Attribute	.93	.62	-.31

Results for the critical attribute (floor) indicated a significant blocking effect (asymptotic $t = -3.38$, $p < .001$) that was not qualified by a statistically significant interaction with the number of brand names used in the experiment (asymptotic $t = -1.82$, $p < .1$). However, the critical attribute had a significantly stronger effect on quality judgments when a larger number of brands was used (asymptotic $t = 3.79$, $p < .001$). This overall competitive commoditization effect was rather strong, as indicated by the fact that the mean feature effect of the critical attribute was higher than the mean feature effect of the brand feature even after brand pre-exposure. This was not the case in any of the previous experiments.

Results for the brand pair that was used in all conditions, Hypalon versus Riken, mirrored the findings for the critical attribute. A significant brand equity preserving effect of pre-exposure was found (asymptotic $t = 3.87$, $p < .001$) that was not qualified by a statistically significant interaction (asymptotic $t = -1.22$, $p = .22$). In addition, an

overall effect of the number of brands used was found indicating a weaker brand effect when a larger number of brands was used (asymptotic $t = -2.35$, $p < .02$).

In sum, the overall effects of the number of brands and of pre-exposure that were predicted by the LMS model were significant and in the predicted direction. However, the LMS model predicted a large interaction in which the effect of the critical attribute on quality judgments is dependent on the interaction between pre-exposure and the number of brands coupled with each critical attribute. This effect, though in the expected direction, was only marginally significant.

In terms of the generalizability of the effects of pre-exposure to situations in which consumers experience multiple branded products that share the same critical attributes, results suggest that although brand equity will decrease and attribute equity will increase due to a competitive commoditization effect, the pre-exposure effect will still obtain.[3] Thus, providing consumers with brand and outcome information before they receive information about critical attributes has a brand equity preserving effect even when multiple brands share the same critical attributes.

The absence of a significant interaction of the blocking effect with the number of brands sharing each level of the critical attribute did not confirm the prediction based on the LMS model. However, this null effect finding does not provide sufficient basis for the dismissal of the LMS account, especially given the fact that the results were directionally consistent with the hypothesized interaction. Nevertheless, the strong competitive commoditization effect and the lack of a significant interaction are more consistent with the predictions based on the "breadth" and "simplicity" criteria of

---

[3] Follow-up contrast analyses showed significant effects of pre-exposure on brand and critical attribute

explanatory coherence (e.g., Thagard 1989). This raises the possibility that the blocking effect and other results found in the previous experiments may be explained in terms of consumers' use of "principles" or "criteria" of explanatory coherence. For example, if consumers are "principled misers" who try to find the simplest and broadest "explanation" for consumption outcomes without going through a "scientific" reasoning process, they may choose the explanation that is present most often when the outcome is present. In the blocking scenario, this would be the A cue and not the B cue. Thus, a "principled miser" account could explain the blocking effect. Experiment 7 was designed to test a "principled miser" account.

---

effects for the One Brand Pair and Two Brand Pair conditions separately (all *ps* < .01).

CHAPTER 9
EXPERIMENT 7

The main focus of Experiment 8 was to test an ad-hoc account of the blocking effect in terms of breadth and simplicity that emerged from the interpretation of the results in Experiment 7. More specifically, it could be argued that people (i) attend to information, encode, and can retrieve the stimulus information, (ii) are cognitive misers in the sense that they prefer to explain events such as consumption outcomes in terms of as few explanatory variables as possible, and (iii) are also cognitive misers in the sense that they do not make inferences about non-presented information and in the sense that the probability that a cue will be chosen as explanation for an event (e.g., the event "high quality") depends directly on the number of occurrences of the event predicted by that cue relative to the number of occurrences predicted by other cues. This should be especially the case when the other explanations do not predict any occurrences that are not also predicted by the "broader" explanation in the blocking paradigm. Finally, the breadth and simplicity account allows for a top-down influence of priors that says that, all else equal, (iv) explanations are more likely to be used to the extent that they are conceptually consistent with previously acquired knowledge in memory. This fourth premisse is similar to the principle of "system coherence" proposed by Thagard (1989).

Experiment 7 tested this "principled miser" account by introducing an extra intrinsic feature (cue X) in Phase 1 of an A+ -> AB+ blocking condition in a two group design. In one group, this extra attribute was perfectly correlated with an unpredictive filler feature

and, hence, was itself unpredictive of the outcome. In the second group, the extra attribute was perfectly correlated with the brand feature and thus perfectly predictive of the outcome (see Table 21 for a design summary).

TABLE 21
EXPERIMENT 7: DESIGN SUMMARY

Type of Extra Phase 1 Attribute (X)	Experimental Phase: Learning 1	Learning 2	Test
Uncorrelated	$A_{br1}X_1$+	$A_{br1}B_{ca1}$+	A?
	$A_{br2}X_1$-	$A_{br2}B_{ca2}$-	B?
	$A_{br1}X_2$+		
	$A_{br2}X_2$-		
Predictive	$A_{br1}X_1$+	$A_{br1}B_{ca1}$+	A?
	$A_{br2}X_2$-	$A_{br2}B_{ca2}$-	B?

Note: Abbreviations used are "br" for "brand," "ca" for "critical attribute," "+" for "high quality," and "-" for "low quality." Subscripts "1" and "2" refer to different levels of a feature.

According to the principled miser account and consistent with the assumption of Independence of Irrelevant Alternatives (IIA), this manipulation should not affect the relative likelihood that subjects would choose the brand feature versus the critical attribute as their "explanation." In both groups, an equally large majority of subjects should base their binary quality judgments on brand names instead of on levels of the critical attribute in a measure that does not include information about the extra attribute. According to the LMS model, however, subjects should be less (more) likely to base their binary quality judgments on brand (critical attribute) information when the extra cue in Phase 1 is predictive of the outcome than when it is not. The brand cue should compete for association strength with a predictive extra cue, but not with an unpredictive extra cue. As a result, the discrepancy between the predicted outcome at the beginning of Phase 2, when the extra cue is no longer presented and the predicted outcome depends

only on the association strength from the brand cue, should be larger when the brand cue had to compete in Phase 1 than when it had not. Therefore, the acquisition of association strength by the critical attribute should be blocked to a lesser degree when the extra cue is predictive than when it is unpredictive. Thus, adding an extra predictive feature in Phase 1 only should facilitate the acquisition of predictive value by the critical attribute in Phase 2.

Method

Fifty-one subjects participated in groups of one to six in a two-cell between-subjects design using the whitewater rafts category. In both cells, subjects received two learning phases with 12 trials in each phase. The features used and their relationship with the outcome were identical to those in the Single Brand Pair-Experimental cell in Experiment 6, with the exception of the addition of an extra feature in Phase 1 but not in Phase 2. This extra feature, "fabric," with levels "polyurethane" and "neoprene," was correlated either with the varying but unpredictive filler feature (country-of-origin) or with the first predictive feature (brand). Thus, the extra feature was either unpredictive or predictive of the outcome (quality). The main dependent measure was the same binary quality judgments measure used in previous experiments consisting of eight profiles constituting a 2 * 2 * 2 factorial combination of brand (Hypalon versus Riken), floor (tubular versus i-beam) and the unpredictive filler feature (made in: Sweden versus Canada). In these eight profiles, no mention was made of the extra feature. Two additional profiles with a binary quality scale were included that had identical levels of brand and the two filler features but differed in their level of the extra feature. Due to experimenter error, data for this measure were gathered from only 46 of 51 subjects.

TABLE 22
EXPERIMENT 7: MEAN FEATURE EFFECTS ON BINARY QUALITY JUDGMENTS

Feature	Mean Feature Effects		
	Uncorrelated Phase 1 Attribute	Predictive Phase 1 Attribute	Difference
Brand	.59	.25	-.34
Critical Attribute	.35	.58	.23

Results and Discussion

The mean feature effects of Brand and Floor depicted in Table 22 disconfirmed the predictions of the "principled miser" account and were consistent with the LMS model's predictions. The introduction of a predictive as opposed to unpredictive extra feature in Phase 1 led to a significant increase in the predictive value of the critical attribute (floor; asymptotic $t = 2.76, p < .01$). The results also showed a significant decrease in the effect of the first predictive feature (brand) when it had been accompanied by a predictive extra feature in Phase 1 instead of an unpredictive extra feature (asymptotic $t = -3.66, p < .001$). Finally, the mean feature effects of the extra feature based on the separate two-profile measure indicated a stronger effect of the extra feature when it was predictive ($MFE = .82$) than when it was not predictive ($MFE = .13$) of the outcome (asymptotic $t = 2.64, p < .01$).

The results in Experiment 7 are inconsistent with an explanation of the results in all seven experiments solely in terms of a rule-based but miserly causal attribution process. However, the results in Experiment 7 are consistent with an associative explanation based on the LMS model.

CHAPTER 10
GENERAL DISCUSSION

The research reported in this dissertation was motivated by a fundamental question: Why do brands have equity? That is, why do consumers base their product evaluations on information about brand names instead of information about the attributes that actually drive product performance? The experiments indicate that one of the reasons why brands have equity is that consumers fail to learn the predictive value of the attributes that actually drive performance. Multiple explorations of potential boundary conditions and moderators in the experiments attest to the robustness of a blocking effect. In addition to the blocking phenomenon, the experiments provide evidence for a number of other phenomena that influence brand equity. The experiments suggest that, in general, brands and attributes compete for equity and that brand equity depends on the number of other brands in a market that share its critical attributes. Finally, the experiments explored four types of process models that could account for the basic blocking effect and that could function as starting points toward a more general theory of how consumers learn to value brands and attributes.

Blocking

When consumers make purchasing decisions, they have to evaluate the consumption outcomes or benefits afforded by the products they are considering. For many products, consumption outcomes cannot be assessed directly in the store. In these situations,

consumers will learn to use search features (e.g., brand names, ingredient information) as indicators of consumption outcomes. When consumers are not aware of the attribute(s) that actually determine product quality, consumers may use the brand name as the main indicator and therefore may show strong brand preferences and be unwilling to try competing products that have the same attributes. However, when consumers are aware of a product's critical attribute(s), and critical attribute and brand information are equally predictive of consumption outcomes, one would expect consumers to rely primarily on attribute information. Intrinsic attributes should have a more direct causal relationship with consumption outcomes. Thus, to the extent that brand equity depends on brand-driven differences in predicted consumption outcomes, brand equity should be decreased when consumers are provided with predictive attribute information relative to the situation in which no predictive attribute information is available.

The prediction that providing consumers with predictive attribute information prompts them to base their outcome evaluations mostly on attribute information is supported by the results from control groups in the experiments. However, this effect did not occur or was strongly attenuated when consumers had just a few experiences with the brand names before being exposed to the predictive intrinsic attribute information. In this situation, consumers's evaluations remained brand-based and the attribute information had little, if any, impact on evaluations. Whereas consumers had little difficulty learning the predictive value of the attribute information when they were exposed to brand and attribute information simultaneously, consumers failed to learn the predictive value of the attribute information when they had been exposed to brand information before receiving attribute information.

Generalizability

The blocking effect was found to be remarkably robust. A strong blocking effect was found in a relatively conservative design containing fewer pre-exposure trials and no explicit trial-by-trial predictions by subjects. This result suggests that the effects of brand pre-exposure on brand and attribute equity do not require many experiences with brand names and consumption outcomes and that the effects are not dependent on active prediction processes during exposure to product information. In addition, the blocking effect was not significantly moderated by doubling the number of attribute exposures from 12 to 24. Thus, one might expect that the effects of pre-exposure on brand and attribute equity will remain strong even when consumers have many product experiences in which they are exposed to attribute information.

Despite the fact that consumers in the simultaneous exposure conditions relied primarily on attribute information, the blocking effect was not significantly stronger when brand information was replaced by information about another intrinsic attribute. This results suggests that although consumers may generally regard attributes as direct causes of consumption outcomes and brands as non-causal predictors, the blocking effect is not moderated by such general beliefs about the causal roles of brands versus attributes. The blocking effect was also not affected by more specific prior beliefs favoring the attribute, although these specific prior beliefs did lead subjects to place more weight on the attribute. These results suggest that although consumers are sensitive to at least some types of prior beliefs, brand pre-exposure will still lead to a blocking effect.

Finally, the blocking effect was not significantly affected by two changes in the basic learning scenario. First, the blocking effect was not moderated by the use of a predictive

positioning attribute (e.g., an "irrelevant attribute") that accompanied the brand information, although the introduction of the positioning attribute did have a strong negative effect on the value subjects placed on brand names. Second, effects of pre-exposure were found even in situations in which several brands shared the same levels of the "blocked" attribute.

In sum, the results strongly suggest that the blocking effect is one reason why consumers place little weight on attribute information and much weight on brand information. The results suggest that as long as consumers are exposed to brand and consumption outcome information before they receive attribute information, they will not learn the predictive value of the attribute information and, as a result, will place little weight on that attribute information when evaluating and choosing products. Such an outcome will lead to suboptimal decisions when cheaper but essentially identical generic products are available.

Cue Competition and Competitive Commoditization

In addition to the blocking effect, two other phenomena were found in the experiments that can influence brand equity. First, results in the control conditions and in the "irrelevant attribute" conditions suggest that cues such as brands and attributes compete for equity.[1] That is, except in blocking scenarios, providing consumers with information about an extra predictive feature will lead to a decrease in the equity of other, equally predictive features. With regard to cue competition between brands and attributes, the results suggest that attributes will often acquire more equity than brands

---

[1] Note that the blocking effect can itself be framed as an instance of cue competition (e.g., Waldmann and Holyoak 1992). The A cue "outcompetes" the B cue in the blocking paradigm because the A cue takes

and may even "outcompete" or "overshadow" brands. Thus, the phenomenon of cue competition should generally decrease the advantage to marketers of using positioning attributes and of providing critical attribute information.

The second phenomenon pertains to information about other products in addition to information about non-brand features of the same product. The results in Experiment 6 suggest that the negative effect of providing attribute information on brand equity will be even stronger when consumers receive attribute and outcome information about other products that share the same predictive attributes and that provide similar consumption outcomes.[2] This phenomenon of "competitive commoditization" leads to further decreases in brand premiums and increases in competitive pressure relative to situations in which consumers are exposed to predictive attribute information but in which each brand has its own predictive attribute(s). This result provides another reason why marketers would try to prevent consumers from trying similar competing products.

Associative Theory of Equity Acquisition and Change

The final goal in this dissertation was to explore the process underlying the blocking effect. Four accounts were examined; three accounts that have previously been reported as explanations of the blocking effect and one account that was inspired directly by the results in the first six experiments. The results in Experiments 4 and 5, among others,

---

most of the available association strength before the B cue even enters the competition.

[2] In all other scenarios in the experiments, subjects received no outcome information about similar competing products. Thus, those other scenarios map onto situations in which consumers receive outcome information for one brand, e.g., through consumption experiences, and are then confronted with search features of a similar product. Thus, these scenarios map onto a trial situation. The competitive commoditization scenario maps onto situations in which consumers receive outcome information for multiple similar brands. An example is the situation in which consumers consume similar products from multiple producers.

argue that the basic blocking effect cannot be explained entirely in terms of either the attentional account or the contingency account. Experiment 7 provided no support for the "principled miser" account. The results in those experiments are, however, consistent with an elemental associative account based on the Least Mean Squares (LMS) connectionist model which also can accommodate the basic phenomena of competitive commoditization and of attributes competing with brands for equity.

These findings suggest that product evaluation is driven by associative processes. These findings also suggest that an associative theory can be developed that explains how brands and attributes acquire equity and how equity is updated in different competitive and informational scenarios. A general associative theory of how consumers learn to value brands and attributes allows one to predict the equity or part-worth utilities of brands and attributes in different situations. According to such a theory, consumers' product evaluations and choices are driven by a forward-looking, massively parallel associative system geared at predicting consumption outcomes based on product cues such as brand names and attribute information. Whenever product feature information and consumption outcome information are experienced contiguously, associations are created or updated between the experienced features and outcomes. After these associations are established, exposure to a product feature will automatically lead to an internal evaluative response that translates into predictions or expectations of consumption outcomes. This evaluative response will then determine outcome predictions and choice behavior. The influence of exposure to a particular product feature on the internal response (and therefore, on evaluation and choice), is a direct function of the association strength between that feature and the consumption outcome.

Thus, to the extent that brand equity and brand part-worth utility can be defined operationally as the change in evaluations due to the addition of a brand name to an otherwise identical product (e.g., Farquhar 1989), brand equity and brand part-worth utility are directly determined by the strength of the associations between brands and the consumption outcomes to be evaluated.

The associations that drive evaluation and choice are updated through a constant feedback loop according to a relatively simple learning rule. Evaluative responses to product feature information are compared to "desired" responses generated by consumption outcome information (e.g., outcome information from direct consumption experiences), and associative strengths are updated to reduce the difference between actual and desired responses. The evaluation and updating processes are not based on any conscious reasoning process, and it is unlikely that consumers have any introspective insight into how product features lead to evaluative responses or into how associations are updated. This system does not provide consumers with information about why they like certain products or why they expect those products to afford certain benefits. The associative system can process large amounts of information for many different products, many different product features, and many different product experiences, constantly updating associations that allow the consumer to make performance predictions and choose products. It can do so because it does not rely on a serial reasoning process that is effortful and can handle only one line of thought at any time but instead relies on many parallel connections and a very simple updating process. Despite the simplicity of learning rule and updating process, an associative system could still account for many of the complex behaviors involved in consumer decision making because the interplay of

large numbers of connections can produce complex relations between inputs (product feature information) and outputs (evaluative responses) even when each connection is updated according to the same simple updating rule.

One of the main advantages of such an associative theory is the fact that associative systems can be described with a high level of specificity by simple connectionist models. With the help of these models, specific predictions can be made about the evolution of part-worth utilities or equity over time and under different scenarios with different product experiences. These models can also be used to make specific predictions of product evaluations and product choices.

Dual Systems Theory of Equity Acquisition and Change

Despite the fact that the results were generally consistent with an LMS-based associative model of consumer evaluation and choice, and despite the fact that none of the Experiments provided positively disconfirming evidence against the LMS account, some of the results do seem to suggest that the LMS account may be incomplete. In particular, the absence of a differential blocking effect in Experiment 6 depending on the number of brands sharing an attribute level, and the order effect in Experiment 4 are difficult to explain in terms of an LMS model.[3] These results, as well as the fact that prior beliefs affected product evaluations but did not significantly interact with the blocking effect, seem more consistent with an explanation of consumer evaluation and choice in terms of two independent processes. The results seem to be most consistent with a "Dual Systems Theory" which holds that two systems vie for control of consumers' evaluative responses. One system is a forward-looking, massively parallel, relatively

effortless associative system that could be described by the LMS model. The second system is a reasoning-based, linear system operating on an episodic memory. According to this view, an associative system that was formed earlier in the evolution of the human species (and that is shared with many animals) constantly tells us whether we should fight or flee, when to love or hate a person, and what food to blame for stomach pains (Baker et al. 1996). This system also tells us how good or bad a product is without having to reason about it. Importantly, the data in the experiments suggest that it is this system that causes the basic blocking, brand-equity preserving, and cue competition effects. The existence of such a system and its influence on consumers' evaluations and choices is not inconsistent with the existence of another, evolutionarily more advanced system based on reasoning processes that operate on an episodic memory (Baker et al. 1996). This system does not cause the basic blocking and brand equity preserving effects, but it does produce differential effects of brands versus attributes, of more specific prior beliefs, and of the number of brands that share the same level of a critical attribute. Based on knowledge about the causal relationships between features and outcomes stored in memory and on general principles of causal explanation such as the principles of breadth, simplicity, and "system coherence" Thagard (1989), a linear, conscious, and retrospective reasoning or inference-making process leads to conclusions about how important a certain feature dimension is likely to be. The retrospective system also allows consumers to remember correctly the patterns of co-occurrence between outcomes and product features. It is possible that activating this system by recalling co-occurrence patterns will cause its output to be weighed more heavily in subsequent quality judgments. This may explain

---

[3] See, e.g., Miller, Barnet, and Grahame (1995) for other criticisms of the LMS model.

the order effect on quality judgments in Experiment 4. Finally, the more cognitive system can account for the differences in Experiment 1 between the predictiveness ratings measure and the other measures. It is possible that the low predictiveness ratings for the brand feature were due to the fact that the predictiveness ratings measure triggered more retrospective processing than the inherently more forward-looking evaluations of new, previously unknown products.

According to the dual systems account, consumers making evaluations or choices always have at least one input consisting of a rather intuitive internal evaluative response that is produced by the associative system. In many cases, a second input, a top-down judgment produced by the more cognitive system, also is available. The absence of significant interactions of the blocking and brand equity preserving effects with the effects of the manipulations of general and specific prior beliefs and the number of brands sharing an attribute suggest that the two processes operate independently. These findings are consistent with findings by Heit (1994) who looked at the integration of prior beliefs and data in category learning and showed that the influences of the prior beliefs and the data were statistically independent.

In sum, the experiments provide preliminary evidence for a dual systems theory of how consumers learn to value product features; that is, of how brands and attributes acquire equity and of how equity is updated. One of these systems is a "bottom-up" associative system. The other system is a more cognitive "top-down" system. The data suggest that these systems independently affect product evaluations and choices. However, the data provide insufficient evidence to exclude the possibility that subjects'

evaluations in the experiments were completely driven by a purely associative system. Further research is necessary to address this possibility.

REFERENCES

Aaker, David A. (1991), Managing Brand Equity: Capitalizing on the Value of a Brand Name. New York: The Free Press.

Aaker, David A. and Kevin L. Keller (1990), "Consumer Evaluations of Brand Extensions," Journal of Marketing, 54 (January), 27-41.

Alba, Joseph W., Hutchinson, J. Wesley, and John Lynch, Jr. (1991), "Memory and Decision Making," in Handbook of Consumer Theory and Research, eds. Harold J. Kassarjian and Thomas S. Robertson. Englewood Cliffs, NJ: Prentice-Hall, 1-49.

Allan, Lorraine G. (1993), "Human Contingency Judgments: Rule Based or Associative?," Psychological Bulletin, 114 (3), 435-448.

Alloy, Lauren B. and Naomi Tabachnik (1984), "Assessment of Covariation by Humans and Animals: The Joint Influence of Prior Expectations and Current Situational Information," Psychological Review, 91 (January), 112-149.

Baker, A. G., Pierre Mercier, Frédéric Vallée-Tourangeau, Robert Frank, and Maria Pan (1993), "Selective Associations and Causality Judgments: Presence of a Strong Causal Factor May Reduce Judgments of a Weaker One," Journal of Experimental Psychology: Learning, Memory, and Cognition, 19 (2), 414-432.

———, Robin A. Murphy, and Frédéric Vallée-Tourangeau (1996), "Associative and Normative Models of Causal Induction: Reacting to Versus Understanding Cause," in The Psychology of Learning and Motivation, Volume 34: Causal Learning, eds. David R. Shanks, Douglas L. Medin, and Keith J. Holyoak. San Diego, CA: Academic Press, 1-45.

Barwise, Patrick (1993), "Brand Equity: Snark or Boojum?" International Journal of Research in Marketing, 10 (March), 93-104.

Belk, Russell W. (1988), "Possessions and the Extended Self," Journal of Consumer Research, 15 (September), 139-168.

Broniarczyk, Susan B. and Joseph W. Alba (1994a), "The Importance of the Brand in Brand Extension," Journal of Marketing Research, 31 (May), 214-228.

——— and ——— (1994b), "Theory versus Data in Prediction and Correlation Tasks," Organizational Behavior and Human Decision Processes, 57 (January), 117-139.

Carpenter, Gregory S., Rashi Glazer, and Kent Nakamoto (1994), "Meaningful Brands From Meaningless Differentiation: The Dependence on Irrelevant Attributes," Journal of Marketing Research, 31 (August), 339-350.

Chapman, Gretchen B. (1991), "Trial Order Affects Cue Interaction in Contingency Judgment," Journal of Experimental Psychology: Learning, Memory, and Cognition, 17 (September), 837-854.

——— and Steven J. Robbins (1990), "Cue Interaction in Human Contingency Judgment," Memory and Cognition, 18 (September), 537-545.

Chapman, Loren J. and Jean P. Chapman (1969), "Illusory Correlation as an Obstacle to the Use of Valid Psychodiagnostic Signs," Journal of Abnormal Psychology, 71 (3), 271-280.

Cheng, Patricia W. (1997), "From Covariation to Causation: A Causal Power Theory," Psychological Review, 104 (April), 367-405.

——— and Keith J. Holyoak (1995), "Complex Adaptive Systems as Intuitive Statisticians: Causality, Contingency, and Prediction," in Comparative Approaches to Cognitive Science, eds. Herbert L. Roitblat and Jean-Arcady Meyer. Cambridge, MA: MIT Press, 271-302.

——— and Laura R. Novick (1992), "Covariation in Natural Causal Induction," Psychological Review, 99 (2), 365-382.

———, Jooyong Park, Aaron S. Yarlas, and Keith J. Holyoak (1996), "A Causal-Power Theory of Focal Sets," in The Psychology of Learning and Motivation, Volume 34: Causal Learning, eds. David R. Shanks, Douglas L. Medin, and Keith J. Holyoak. San Diego, CA: Academic Press, 313-355.

Dickinson, Anthony, David Shanks, and John Evenden (1984), "Judgement of Act-Outcome Contingency: The Role of Selective Attribution," Quarterly Journal of Experimental Psychology, 36A (February), 29-50.

Dickson, Peter R. and Alan G. Sawyer (1990), "The Price Knowledge and Search of Supermarket Shoppers," Journal of Marketing, 54 (July), 42-53.

Einhorn, Hillel J. and Robin M. Hogarth (1986), "Judging Probable Cause," Psychological Bulletin, 99 (January), 3-19.

Erdem, Tülin (1997) "An Empirical Analysis of Umbrella Branding," working paper, Haas School of Business, University of California at Berkeley.

——— and Joffre Swait (1998), "Brand Equity as a Signaling Phenomenon," Journal of Consumer Psychology, 7 (2), 131-157.

Farquhar, Peter H. (1989), "Managing Brand Equity," Marketing Research, 1 (September), 24-33.

Feldman, Jack M. and John G. Lynch (1988), "Self-Generated Validity and Other Effects of Measurement of Belief, Attitude, Intention, and Behavior," Journal of Applied Psychology, 73 (3), 421-435.

Gluck, Mark A. and Gordon H. Bower (1988), "From Conditioning to Category Learning: An Adaptive Network Model," Journal of Experimental Psychology: General, 117 (3), 227-247.

Heit, Evan (1994), "Models of the Effects of Prior Knowledge on Category Learning," Journal of Experimental Psychology: Learning, Memory, and Cognition, 20 (6), 1264-1282.

Hoch, Stephen J. and Young-Won Ha (1986), "Consumer Learning: Advertising and the Ambiguity of Product Experience," Journal of Consumer Research, 13 (September), 221-233.

Holyoak, Keith J., Hyunghee Koh, and Richard E. Nisbett (1989), "A Theory of Conditioning: Inductive Learning Within Rule-Based Default Hierarchies," Psychological Review, 96 (April), 315-340.

Hoyer, Wayne D. (1984), "An Examination of Consumer Decision Making for a Common Repeat-Purchase Product," Journal of Consumer Research, 11 (December), 822-829.

Kamakura, Wagner A. and Gary J. Russell (1993), "Measuring Brand Value With Scanner Data," International Journal of Research in Marketing, 10 (March), 9-22.

Kamin, Leon J. (1969), "Predictability, Surprise, Attention, and Conditioning," in Punishment and Aversive Behavior, eds. Byron A. Campbell and Russell M. Church. New York: Appleton-Century-Crofts, 279-296.

Keller, Kevin L. (1993), "Conceptualizing, Measuring, and Managing Customer-Based Brand Equity," Journal of Marketing, 57 (January), 1-22.

——— and David A. Aaker (1992), "The Effects of Sequential Introduction of Brand Extensions," Journal of Marketing Research, 29 (February), 35-50.

Kelley, Harold H. (1973), "The Process of Causal Attribution," American Psychologist, 28 (February), 107-128.

Kleine, Robert E., III, Susan Schultz Kleine, and Jerome B. Kernan (1993), "Mundane Consumption and the Self: A Social-Identity Perspective," Journal of Consumer Psychology, 2 (3), 209-235.

Kruschke John K. (1992), "ALCOVE: An Exemplar-Based Connectionist Model of Category Learning," Psychological Review, 99 (1), 22-44.

——— (1996), "Base Rates in Category Learning," Journal of Experimental Psychology: Learning, Memory, and Cognition, 22 (January), 3-26.

Leong, Siew Meng (1993), "Consumer Decision Making for Common, Repeat-Purchase Products: A Dual Replication," Journal of Consumer Psychology, 2 (2), 193-208.

Lopez, Francisco J., David R. Shanks, Julian Almaraz, and Pablo Fernandez (1998), "Effects of Trial Order on Contingency Judgments: A Comparison of Associative and Probabilistic Contrast Accounts," Journal of Experimental Psychology: Learning, Memory, and Cognition, 24 (May), 672-694.

Lynch, John G. Jr., Howard Marmorstein, and Michael F. Weigold (1988), "Choices from Sets Including Remembered Brands: Use of Recalled Attributes and Prior Overall Evaluations," Journal of Consumer Research, 15 (September), 169-184.

McClelland, James L., David E. Rumelhart (1986), and the PDP Research Group (1986), Parallel Distributed Processing: Explorations in the Microstructure of Cognition, Volume 2: Psychological and Biological Models. Cambridge, MA: MIT Press.

McSweeney, Frances K. and Calvin Bierley (1984), "Recent Developments in Classical Conditioning," Journal of Consumer Research, 11 (September), 619-631.

Melz, Eric R., Patricia W. Cheng, Keith J. Holyoak, and Michael R. Waldmann (1993), "Cue Competition in Human Categorization: Contingency or the Rescorla-Wagner Learning Rule? Comment on Shanks (1991)," Journal of Experimental Psychology: Learning, Memory, and Cognition, 19 (November), 1398-1410.

Miller, Ralph R., Robert C. Barnet, and Nicholas J. Grahame (1995), "Assessment of the Rescorla-Wagner Model," Psychological Bulletin, 117 (May), 363-386.

Montgomery, Cynthia A. and Birger Wernerfelt (1992), "Risk Reduction and Umbrella Branding," Journal of Business, 65 (January), 31-50.

Nisbett, Richard E. and Lee Ross (1980), Human Inference: Strategies and Shortcomings of Social Judgment. Englewood Cliffs, NJ: Prentice-Hall.

Nosofsky, Robert M. (1986), "Attention, Similarity, and the Identification-Categorization Relationship," Journal of Experimental Psychology: General, 115, 39-57.

Park, Chan Su and V. Srinivasan (1994), "A Survey-Based Method for Measuring and Understanding Brand Equity and Its Extendibility," Journal of Marketing Research, 31 (May), 271-288.

Park, C. Whan, Sandra Milberg, and Robert Lawson (1991), "Evaluation of Brand Extensions: The Role of Product Feature Similarity and Brand Concept Consistency," Journal of Consumer Research, 18 (September), 185-193.

Pavlov, Ivan P. (1927), Conditioned Reflexes: An Investigation of the Physiological Activity of the Cerebral Cortex, Trans. G. V. Anrep, 1960. New York: Dover.

Pearce, John M. (1994), "Similarity and Discrimination: A Selective Review and a Connectionist Model," Psychological Review, 101 (October), 587-607.

Price, Paul C. and J. Frank Yates (1995), "Associative and Rule-Based Accounts of Cue Interaction in Contingency Judgment," Journal of Experimental Psychology: Learning, Memory, and Cognition, 21 (November), 1639-1655.

Read, Stephen J. and Amy Marcus-Newhall (1993), "Explanatory Coherence in Social Explanations: A Parallel Distributed Processing Account," Journal of Social and Personality Psychology, 65 (3), 429-447.

Rescorla, Robert A. and Allan R. Wagner (1972), "A Theory of Pavlovian Conditioning: Variations in the Effectiveness of Reinforcement and Nonreinforcement," in Classical Conditioning II: Current Research and Theory, eds. Abraham H. Black and William F. Prokasy. New York: Appleton-Century-Crofts, 64-99.

Richins, Marsha L. (1994), "Valuing Things: The Public and Private Meanings of Possessions," Journal of Consumer Research, 21 (December), 504-521.

Rumelhart, David E., James L. McClelland, and the PDP Research Group (1986), Parallel Distributed Processing: Explorations in the Microstructure of Cognition, Volume 1: Foundations. Cambridge, MA: MIT Press.

Sanbonmatsu, David M., Sharon A. Akimoto, and Bryan D. Gibson (1994), "Stereotype-Based Blocking in Social Explanation," Personality and Social Psychology Bulletin, 20 (February), 71-81.

Sappington, David E. M. and Birger Wernerfelt (1985), "To Brand or Not to Brand? A Theoretical and Empirical Question," Journal of Business, 58 (July), 279-293.

Schlenker, Barry R. (1980), Impression Management: The Self-Concept, Social Identity, and Interpersonal Relations. Monterey, CA: Brooks/Cole.

Schneider, W. (1988), "Micro Experimental Laboratory: An Integrated System for IBM PC Compatibles," Behavior Research Methods, Instruments, and Computers, 20, 206-217.

Shaklee, Harriet and Baruch Fischhoff (1982), "Strategies of Information Search in Causal Analysis," Memory and Cognition, 10 (6), 520-530.

Shanks, David R. (1985), "Forward and Backward Blocking in Human Contingency Judgement," Quarterly Journal of Experimental Psychology, 37B (February), 1-21.

——— (1991), "Categorization by a Connectionist Network," Journal of Experimental Psychology: Learning, Memory, and Cognition, 17 (May), 433-443.

——— (1993a), "Associative Versus Contingency Accounts of Category Learning: Reply to Melz, Cheng, Holyoak, and Waldmann (1993)," Journal of Experimental Psychology: Learning, Memory, and Cognition, 19 (November), 1411-1423.

——— (1993b), "Human Instrumental Learning: A Critical Review of Data and Theory," British Journal of Psychology, 84, 319-354.

——— (1994), "Human Associative Learning," in Animal Learning and Cognition, ed. Nicholas J. Mackintosh. San Diego, CA: Academic Press, 335-374.

——— and Anthony Dickinson (1987), "Associative Accounts of Causality Judgment", in The Psychology of Learning and Motivation, Volume 21, ed. Gordon H. Bower, San Diego, CA: Academic Press, 229-260.

———, Francisco J. Lopez, Richard J. Darby, and Anthony Dickinson (1996), "Distinguishing Associative and Probabilistic Contrast Theories of Human Contingency Judgment," in The Psychology of Learning and Motivation, Volume 34: Causal Learning, eds. David R. Shanks, Douglas L. Medin, and Keith J. Holyoak. San Diego, CA: Academic Press, 265-311.

———, Douglas L. Medin, and Keith J. Holyoak, eds. (1996), The Psychology of Learning and Motivation, Volume 34: Causal Learning. San Diego, CA: Academic Press.

Simon, Carol J. and Mary W. Sullivan (1993), "The Measurement and Determinants of Brand Equity: A Financial Approach," Marketing Science, 12 (Winter), 28-52.

Smith, Eliot R. (1996), "What Do Connectionism and Social Psychology Offer Each Other?," Journal of Personality and Social Psychology, 70 (May), 893-912.

Spellman, Barbara A. (1996a), "Acting As Intuitive Scientists: Contingency Judgments Are Made While Controlling for Alternative Potential Causes," Psychological Science, 7 (November), 337-342.

——— (1996b), "Conditionalizing Causality," in The Psychology of Learning and Motivation, Volume 34: Causal Learning, eds. David R. Shanks, Douglas L. Medin, and Keith J. Holyoak. San Diego, CA: Academic Press, 167-206.

Srinivasan, V. (1979), "Network Models for Estimating Brand-Specific Effects in Multi-Attribute Marketing Models," Management Science, 25 (January), 11-21.

Sullivan, Mary W. (1990), "Measuring Image Spillovers in Umbrella-Branded Products," Journal of Business, 63 (July), 309-329.

——— (1992), "Brand Extensions: When to Use Them," Management Science, 38 (June), 793-805.

Swait, Joffre, Tülin Erdem, Jordan Louviere, and Chris Dubelaar (1993), "The Equalization Price: A Measure of Consumer-Perceived Brand Equity," International Journal of Research in Marketing, 10 (March), 23-45.

Thagard, Paul (1989), "Explanatory Coherence," Behavioral and Brain Sciences, 12, 435-467.

Tversky, Amos (1977), "Features of Similarity," Psychological Review, 84 (July), 327-352.

Van Hamme, Linda J. and Edward A. Wasserman (1994), "Cue Competition in Causality Judgments: The Role of Nonrepresentation of Compound Stimulus Elements," Learning and Motivation, 25 (May), 127-151.

Van Overwalle, Frank (1998), "Causal Explanation as Constraint Satisfaction: A Critique and a Feedforward Connectionist Alternative," Journal of Personality and Social Psychology, 74 (February), 312-328.

Wagner, Allan R. (1969), "Stimulus Selection and a "Modified Continuity Theory," in The Psychology of Learning and Motivation, Vol. 3, eds. Gordon H. Bower and Janet Taylor Spence. New York: Academic Press, 1-41.

———, Frank A. Logan, Karl Haberlandt, and Trevor Price (1968), "Stimulus Selection in Animal Discrimination Learning," Journal of Experimental Psychology, 76 (2), 171-180.

Waldmann, Michael R. (1996), "Knowledge-Based Causal Induction," in The Psychology of Learning and Motivation, Volume 34: Causal Learning, eds. David R. Shanks, Douglas L. Medin, and Keith J. Holyoak. San Diego, CA: Academic Press, 47-88.

——— and Keith J. Holyoak (1992), "Predictive and Diagnostic Learning Within Causal Models: Asymmetries in Cue Competition," Journal of Experimental Psychology: Learning, Memory, and Cognition, 121 (March), 222-236.

Wasserman, Edward . A. (1990), "Detecting Response-Outcome Relations: Toward an Understanding of the Causal Texture of the Environment", in The Psychology of Learning and Motivation, Volume 26, ed. Gordon H. Bower. San Diego, CA: Academic Press, 27-82.

Wasserman, Edward A. and Ralph R. Miller (1997), "What's Elementary about Associative Learning," Annual Review of Psychology, 48, 573-607.

Wernerfelt, Birger (1988), "Umbrella Branding as a Signal of New Product Quality: An Example of Signalling by Posting a Bond," RAND Journal of Economics, 19 (3), 458-466.

White, Peter A. (1995), "Use of Prior Beliefs in the Assignment of Causal Roles: Causal Powers versus Regularity-Based Accounts," Memory & Cognition, 23 (March), 243-254.

Widrow, G, and M. E. Hoff (1960), "Adaptive Switching Circuits," in Institute of Radio Engineers, Western Electronic Show and Convention, Convention Record (Part 4), 96-104.

Young, Michael E. (1995), "On the Origin of Personal Causal Theories," Psychonomic Bulletin and Review, 2 (March), 83-104.

APPENDIX A
INSTRUCTIONS TO SUBJECTS

Instructions Experiment 1

Instructions

This experiment is part of a research program in the Marketing Department about how consumers learn about unfamiliar products. Consumers are regularly confronted with classes of products that they know very little about. This can occur when new types of products are invented, such as video players a few decades ago, or personal digital assistants (PDAs) today. Products can also be unfamiliar because they belong to a different stage in life (e.g., baby food for most college students) or because they relate to specialist hobbies or interests (e.g., photo cameras or whitewater rafting equipment). When consumers get interested in such products, they need to learn how to predict good quality.

In this experiment, you will get information about whitewater rafts that are available on the market. (As you may or may not know, whitewater rafts are inflatable boats that are used to navigate wild rivers). You will see a profile for each raft that includes a variety of information. Included in this information will be the quality level of each raft (HIGH or LOW). In addition, each raft will have a catalog identification number. Your task as a consumer is to learn how to predict the quality level of whitewater rafts. In other words,

your task as an unfamiliar consumer is to determine what really matters in whitewater rafts.

If you need any clarification, please ask at this time. If you do not have questions, press SPACEBAR to start.

Instructions Experiments 2, 2a, and 4

Instructions

This experiment is part of a research program in the Marketing Department about how consumers learn about unfamiliar products. Consumers are regularly confronted with classes of products that they know very little about. This can occur when new types of products are invented, such as video players a few decades ago, or personal digital assistants (PDAs) today. Products can also be unfamiliar because they don't match your stage in life (e.g., baby food for most college students) or because they relate to specialized hobbies or interests (e.g., photo cameras or whitewater rafting equipment). When consumers get interested in such products, they need to learn how to predict the performance of the products. For example, you might become interested in purchasing a whitewater raft. Because you are planning to use it in wild rivers, you are looking for a raft that has good steering performance. When shopping, you can't really test the steering performance on the water so you need to make your decision based on other information. Press SPACEBAR when you are ready to see the next screen.

In this experiment, you will get information about whitewater rafts that are available on the market. You will see a profile for each raft that includes a variety of information. Included in this information will be the steering performance of each raft (GOOD or BAD steering performance). In addition, each raft will have a catalog number to identify different varieties. Your task as a consumer is to learn how to predict the steering performance of whitewater rafts. In other words, your task as a consumer is to learn how to distinguish between good steering and bad steering based on the written descriptions. If you need any clarification, please ask at this time. If you do not have questions, press SPACEBAR to see the first raft.

<u>Instructions Experiment 3</u>

Instructions

In this experiment, you will get information about down jackets that are available on the British market. You will see a profile for each down jacket that includes a variety of information. Included in this information will be the quality level of each jacket (HIGH or LOW quality). Each jacket will also have a model number. Your task as a consumer is to learn how to predict the quality level of down jackets. In other words, your task as a consumer is to learn how to distinguish between high quality and low quality.

If you need any clarification, please ask at this time. If you do not have questions, press SPACEBAR to see the first down jacket.

Instructions Experiment 5

Instructions

This experiment is part of a research program in the Marketing Department about how consumers learn about unfamiliar products. Consumers are regularly confronted with classes of products that they know very little about. This can occur when new types of products are invented, such as video players a few decades ago, or personal digital assistants (PDAs) today. Products can also be unfamiliar because they don't match your stage in life (e.g., baby food for most college students) or because they relate to specialized hobbies or interests (e.g., photo cameras or whitewater rafting equipment). When consumers get interested in such products, they need to learn how to predict the performance of the products. For example, you might become interested in purchasing a whitewater raft. Because you are planning to use it on wild rivers, you are looking for a high quality raft. When shopping, you can't really test the quality on the water so you need to make your decision based on other information.

Press SPACEBAR when you are ready to see the next screen.

In this experiment, you will get information about whitewater rafts that are available on the market. You will see profiles of rafts that include a variety of information. Included in this information will be the quality level of the raft (HIGH or LOW quality). In addition, each raft profile will show a catalog number. Your task as a consumer is to learn how to predict the quality level of whitewater rafts. In other words, your task as a

consumer is to learn how to distinguish between high quality and low quality based on the written descriptions.

If you need any clarification, please ask at this time. If you do not have questions, press SPACEBAR to see the first raft.

Instructions Experiments 6 and 7

Instructions

This experiment is part of a research program in the Marketing Department about how consumers learn about unfamiliar products. Consumers are regularly confronted with classes of products that they know very little about. This can occur when new types of products are invented, such as video players a few decades ago, or personal digital assistants (PDAs) today. Products can also be unfamiliar because they don't match your stage in life (e.g., baby food for most college students) or because they relate to specialized hobbies or interests (e.g., photo cameras or whitewater rafting equipment). When consumers get interested in such products, they need to learn how to predict the performance of the products. For example, you might become interested in purchasing a whitewater raft. Because you are planning to use it in wild rivers, you are looking for a high quality raft. When shopping, you can't really test the quality on the water so you need to make your decision based on other information.

Press SPACEBAR when you are ready to see the next screen.

In this experiment, you will get information about whitewater rafts that are available on the market. You will see a profile for each raft that includes a variety of information. Included in this information will be the quality level of each raft (HIGH or LOW quality). In addition, each raft will have a catalog number to identify different varieties. Your task as a consumer is to learn how to predict the quality level of whitewater rafts. In other words, your task as a consumer is to learn how to distinguish between high quality and low quality based on the written descriptions.

If you need any clarification, please ask at this time. If you do not have questions, press SPACEBAR to see the first raft.

APPENDIX B
PRODUCT PROFILES

First Learning Phase

Trial 1

Whitewater Raft nr. S541
* **BRAND: HYPALON**
* COMPARTMENTS: AIRECELL
* HULL: POLYURETHANE

This raft is **HIGH QUALITY**

Trial 2

Whitewater Raft nr. S223
* **BRAND: RIKEN**
* COMPARTMENTS: AIRECELL
* HULL: POLYURETHANE

This raft is **LOW QUALITY**

Trial 3

Whitewater Raft nr. S316
* **BRAND: HYPALON**
* COMPARTMENTS: CLOSED-CELL
* HULL: POLYURETHANE

This raft is **HIGH QUALITY**

Trial 4

Whitewater Raft nr. S853
* **BRAND: RIKEN**
* COMPARTMENTS: CLOSED-CELL
* HULL: POLYURETHANE

This raft is **LOW QUALITY**

Second Learning Phase

Trial 1 (repeated 3 times)

Whitewater Raft nr. S379
* **BRAND: HYPALON**
* COMPARTMENTS: AIRECELL
* HULL: POLYURETHANE
* **FLOOR: TUBULAR**

This raft is **HIGH QUALITY**

Trial 2 (repeated 3 times)

Whitewater Raft nr. S714
* **BRAND: RIKEN**
* COMPARTMENTS: AIRECELL
* HULL: POLYURETHANE
* **FLOOR: I-BEAM**

This raft is **LOW QUALITY**

Trial 3 (repeated 3 times)

Whitewater Raft nr. S684
* **BRAND: HYPALON**
* COMPARTMENTS: CLOSED-CELL
* HULL: POLYURETHANE
* **FLOOR: TUBULAR**

This raft is **HIGH QUALITY**

Trial 4 (repeated 3 times)

Whitewater Raft nr. S169
* **BRAND: RIKEN**
* COMPARTMENTS: CLOSED-CELL
* HULL: POLYURETHANE
* **FLOOR: I-BEAM**

This raft is **LOW QUALITY**

FIGURE B-1
STIMULI EXPERIMENT 1: BRAND AS FIRST PREDICTIVE FEATURE

First Learning Phase

Trial 1

Whitewater Raft nr. S541
* **FRAME: HYPALON**
* COMPARTMENTS: AIRECELL
* HULL: POLYURETHANE

This raft is **HIGH QUALITY**

Trial 2

Whitewater Raft nr. S223
* **FRAME: RIKEN**
* COMPARTMENTS: AIRECELL
* HULL: POLYURETHANE

This raft is **LOW QUALITY**

Trial 3

Whitewater Raft nr. S316
* **FRAME: HYPALON**
* COMPARTMENTS: CLOSED-CELL
* HULL: POLYURETHANE

This raft is **HIGH QUALITY**

Trial 4

Whitewater Raft nr. S853
* **FRAME: RIKEN**
* COMPARTMENTS: CLOSED-CELL
* HULL: POLYURETHANE

This raft is **LOW QUALITY**

Second Learning Phase

Trial 1 (repeated 3 times)

Whitewater Raft nr. S379
* **FRAME: HYPALON**
* COMPARTMENTS: AIRECELL
* HULL: POLYURETHANE
* **FLOOR: TUBULAR**

This raft is **HIGH QUALITY**

Trial 2 (repeated 3 times)

Whitewater Raft nr. S714
* **FRAME: RIKEN**
* COMPARTMENTS: AIRECELL
* HULL: POLYURETHANE
* **FLOOR: I-BEAM**

This raft is **LOW QUALITY**

Trial 3 (repeated 3 times)

Whitewater Raft nr. S684
* **FRAME: HYPALON**
* COMPARTMENTS: CLOSED-CELL
* HULL: POLYURETHANE
* **FLOOR: TUBULAR**

This raft is **HIGH QUALITY**

Trial 4 (repeated 3 times)

Whitewater Raft nr. S169
* **FRAME: RIKEN**
* COMPARTMENTS: CLOSED-CELL
* HULL: POLYURETHANE
* **FLOOR: I-BEAM**

This raft is **LOW QUALITY**

FIGURE B-2
STIMULI EXPERIMENT 1: ATTRIBUTES AS FIRST PREDICTIVE FEATURE

First Learning Phase	**Second Learning Phase**
Trial 1	*Trial 1 (repeated 3 or 6 times)*
Whitewater Raft nr. S541 * **BRAND: HYPALON** * MADE IN: SWEDEN * WARRANTY: 1 YEAR This raft is **HIGH QUALITY**	Whitewater Raft nr. S379 * **BRAND: HYPALON** * MADE IN: SWEDEN * WARRANTY: 1 YEAR * **RUDDER: FOURCASE** This raft is **HIGH QUALITY**
Trial 2	*Trial 2 (repeated 3 or 6 times)*
Whitewater Raft nr. S223 * **BRAND: RIKEN** * MADE IN: SWEDEN * WARRANTY: 1 YEAR This raft is **LOW QUALITY**	Whitewater Raft nr. S714 * **BRAND: RIKEN** * MADE IN: SWEDEN * WARRANTY: 1 YEAR * **RUDDER: BACKBAR** This raft is **LOW QUALITY**
Trial 3	*Trial 3 (repeated 3 or 6 times)*
Whitewater Raft nr. S316 * **BRAND: HYPALON** * MADE IN: CANADA * WARRANTY: 1 YEAR This raft is **HIGH QUALITY**	Whitewater Raft nr. S684 * **BRAND: HYPALON** * MADE IN: CANADA * WARRANTY: 1 YEAR * **RUDDER: FOURCASE** This raft is **HIGH QUALITY**
Trial 4	*Trial 4 (repeated 3 or 6 times)*
Whitewater Raft nr. S853 * **BRAND: RIKEN** * MADE IN: CANADA * WARRANTY: 1 YEAR This raft is **LOW QUALITY**	Whitewater Raft nr. S169 * **BRAND: RIKEN** * MADE IN: CANADA * WARRANTY: 1 YEAR * **RUDDER: BACKBAR** This raft is **LOW QUALITY**

FIGURE B-3
STIMULI EXPERIMENT 2: LOW PRIOR BELIEFS

First Learning Phase	Second Learning Phase
Trial 1	*Trial 1 (repeated 3 or 6 times)*
Whitewater Raft nr. S541 *** BRAND: HYPALON** * MADE IN: SWEDEN * WARRANTY: 1 YEAR This raft has **GOOD STEERING PERFORMANCE**	Whitewater Raft nr. S379 *** BRAND: HYPALON** * MADE IN: SWEDEN * WARRANTY: 1 YEAR *** RUDDER: FOURCASE** This raft has **GOOD STEERING PERFORMANCE**
Trial 2	*Trial 2 (repeated 3 or 6 times)*
Whitewater Raft nr. S223 *** BRAND: RIKEN** * MADE IN: SWEDEN * WARRANTY: 1 YEAR This raft has **BAD STEERING PERFORMANCE**	Whitewater Raft nr. S714 *** BRAND: RIKEN** * MADE IN: SWEDEN * WARRANTY: 1 YEAR *** RUDDER: BACKBAR** This raft has **BAD STEERING PERFORMANCE**
Trial 3	*Trial 3 (repeated 3 or 6 times)*
Whitewater Raft nr. S316 *** BRAND: HYPALON** * MADE IN: CANADA * WARRANTY: 1 YEAR This raft has **GOOD STEERING PERFORMANCE**	Whitewater Raft nr. S684 *** BRAND: HYPALON** * MADE IN: CANADA * WARRANTY: 1 YEAR *** RUDDER: FOURCASE** This raft has **GOOD STEERING PERFORMANCE**
Trial 4	*Trial 4 (repeated 3 or 6 times)*
Whitewater Raft nr. S853 *** BRAND: RIKEN** * MADE IN: CANADA * WARRANTY: 1 YEAR This raft has **BAD STEERING PERFORMANCE**	Whitewater Raft nr. S169 *** BRAND: RIKEN** * MADE IN: CANADA * WARRANTY: 1 YEAR *** RUDDER: BACKBAR** This raft has **BAD STEERING PERFORMANCE**

FIGURE B-4
STIMULI EXPERIMENT 2: HIGH PRIOR BELIEFS

First Learning Phase

Trial 1

Down Jacket nr. S541
* **BRAND: HYPALON**
* STITCHING: EXTRA TIGHT
* COVER MATERIAL: COTTON

This jacket is **HIGH QUALITY**

Trial 2

Down Jacket nr. S223
* **BRAND: RIKEN**
* STITCHING: EXTRA TIGHT
* COVER MATERIAL: COTTON

This jacket is **LOW QUALITY**

Trial 3

Down Jacket nr. S316
* **BRAND: HYPALON**
* STITCHING: EXTRA TIGHT
* COVER MATERIAL: SYNTHETIC

This jacket is **HIGH QUALITY**

Trial 4

Down Jacket nr. S853
* **BRAND: RIKEN**
* STITCHING: EXTRA TIGHT
* COVER MATERIAL: SYNTHETIC

This jacket is **LOW QUALITY**

Second Learning Phase

Trial 1 (repeated 3 times)

Down Jacket nr. S379
* **BRAND: HYPALON**
* STITCHING: EXTRA TIGHT
* COVER MATERIAL: COTTON
* **FILL RATING: 550**

This jacket is **HIGH QUALITY**

Trial 2 (repeated 3 times)

Down Jacket nr. S714
* **BRAND: RIKEN**
* STITCHING: EXTRA TIGHT
* COVER MATERIAL: COTTON
* **FILL RATING: 500**

This jacket is **LOW QUALITY**

Trial 3 (repeated 3 times)

Down Jacket nr. S684
* **BRAND: HYPALON**
* STITCHING: EXTRA TIGHT
* COVER MATERIAL: SYNTHETIC
* **FILL RATING: 550**

This jacket is **HIGH QUALITY**

Trial 4 (repeated 3 times)

Down Jacket nr. S169
* **BRAND: RIKEN**
* STITCHING: EXTRA TIGHT
* COVER MATERIAL: SYNTHETIC
* **FILL RATING: 500**

This jacket is **LOW QUALITY**

FIGURE B-5
STIMULI EXPERIMENT 3: BRAND-ONLY

First Learning Phase	Second Learning Phase
Trial 1	*Trial 1 (repeated 3 times)*
Down Jacket nr. S541 *** BRAND: HYPALON** *** DOWN FILL: ALPINE CLASS** * STITCHING: EXTRA TIGHT * COVER MATERIAL: COTTON This jacket is **HIGH QUALITY**	Down Jacket nr. S379 *** BRAND: HYPALON** *** DOWN FILL: ALPINE CLASS** * STITCHING: EXTRA TIGHT * COVER MATERIAL: COTTON *** FILL RATING: 550** This jacket is **HIGH QUALITY**
Trial 2	*Trial 2 (repeated 3 times)*
Down Jacket nr. S223 *** BRAND: RIKEN** *** DOWN FILL: REGULAR** * STITCHING: EXTRA TIGHT * COVER MATERIAL: COTTON This jacket is **LOW QUALITY**	Down Jacket nr. S714 *** BRAND: RIKEN** *** DOWN FILL: REGULAR** * STITCHING: EXTRA TIGHT * COVER MATERIAL: COTTON *** FILL RATING: 500** This jacket is **LOW QUALITY**
Trial 3	*Trial 3 (repeated 3 times)*
Down Jacket nr. S316 *** BRAND: HYPALON** *** DOWN FILL: ALPINE CLASS** * STITCHING: EXTRA TIGHT * COVER MATERIAL: SYNTHETIC This jacket is **HIGH QUALITY**	Down Jacket nr. S684 *** BRAND: HYPALON** *** DOWN FILL: ALPINE CLASS** * STITCHING: EXTRA TIGHT * COVER MATERIAL: SYNTHETIC *** FILL RATING: 550** This jacket is **HIGH QUALITY**
Trial 4	*Trial 4 (repeated 3 times)*
Down Jacket nr. S853 *** BRAND: RIKEN** *** DOWN FILL: REGULAR** * STITCHING: EXTRA TIGHT * COVER MATERIAL: SYNTHETIC This jacket is **LOW QUALITY**	Down Jacket nr. S169 *** BRAND: RIKEN** *** DOWN FILL: REGULAR** * STITCHING: EXTRA TIGHT * COVER MATERIAL: SYNTHETIC *** FILL RATING: 500** This jacket is **LOW QUALITY**

FIGURE B-6
STIMULI EXPERIMENT 3: BRAND PLUS "IRRELEVANT ATTRIBUTE"

First Learning Phase	**Second Learning Phase**
Trial 1 (repeated 3 times)	*Trial 1 (repeated 3 times)*
Whitewater Raft nr. S541 *** BRAND: HYPALON** * MADE IN: SWEDEN * WARRANTY: 1 YEAR This raft is **HIGH QUALITY**	Whitewater Raft nr. S379 *** BRAND: HYPALON** * RUDDER: FOURCASE * FABRIC: POLYURETHANE *** FLOOR: TUBULAR** This raft is **HIGH QUALITY**
Trial 2 (repeated 3 times)	*Trial 2 (repeated 3 times)*
Whitewater Raft nr. S223 *** BRAND: RIKEN** * MADE IN: SWEDEN * WARRANTY: 1 YEAR This raft is **LOW QUALITY**	Whitewater Raft nr. S714 *** BRAND: RIKEN** * RUDDER: FOURCASE * FABRIC: POLYURETHANE *** FLOOR: I-BEAM** This raft is **LOW QUALITY**
Trial 3 (repeated 3 times)	*Trial 3 (repeated 3 times)*
Whitewater Raft nr. S316 *** BRAND: HYPALON** * MADE IN: CANADA * WARRANTY: 1 YEAR This raft is **HIGH QUALITY**	Whitewater Raft nr. S684 *** BRAND: HYPALON** * RUDDER: BACKBAR * FABRIC: POLYURETHANE *** FLOOR: TUBULAR** This raft is **HIGH QUALITY**
Trial 4 (repeated 3 times)	*Trial 4 (repeated 3 times)*
Whitewater Raft nr. S853 *** BRAND: RIKEN** * MADE IN: CANADA * WARRANTY: 1 YEAR This raft is **LOW QUALITY**	Whitewater Raft nr. S169 *** BRAND: RIKEN** * RUDDER: BACKBAR * FABRIC: POLYURETHANE *** FLOOR: I-BEAM** This raft is **LOW QUALITY**

FIGURE B-7
STIMULI EXPERIMENT 5

First Learning Phase	Second Learning Phase
Trial 1 (repeated 2 times)	*Trial 1 (repeated 4 times)*
Whitewater Raft nr. S541 *** BRAND: HYPALON** * MADE IN: SWEDEN * WARRANTY: 1 YEAR This raft is **HIGH QUALITY**	Whitewater Raft nr. S379 *** BRAND: HYPALON** * MADE IN: SWEDEN * WARRANTY: 1 YEAR *** FLOOR: TUBULAR** This raft is **HIGH QUALITY**
Trial 2 (repeated 2 times)	*Trial 2 (repeated 4 times)*
Whitewater Raft nr. S223 *** BRAND: RIKEN** * MADE IN: SWEDEN * WARRANTY: 1 YEAR This raft is **LOW QUALITY**	Whitewater Raft nr. S714 *** BRAND: RIKEN** * MADE IN: SWEDEN * WARRANTY: 1 YEAR *** FLOOR: I-BEAM** This raft is **LOW QUALITY**
Trial 3 (repeated 2 times)	*Trial 3 (repeated 4 times)*
Whitewater Raft nr. S316 *** BRAND: HYPALON** * MADE IN: CANADA * WARRANTY: 1 YEAR This raft is **HIGH QUALITY**	Whitewater Raft nr. S684 *** BRAND: HYPALON** * MADE IN: CANADA * WARRANTY: 1 YEAR *** FLOOR: TUBULAR** This raft is **HIGH QUALITY**
Trial 4 (repeated 2 times)	*Trial 4 (repeated 4 times)*
Whitewater Raft nr. S853 *** BRAND: RIKEN** * MADE IN: CANADA * WARRANTY: 1 YEAR This raft is **LOW QUALITY**	Whitewater Raft nr. S169 *** BRAND: RIKEN** * MADE IN: CANADA * WARRANTY: 1 YEAR *** FLOOR: I-BEAM** This raft is **LOW QUALITY**

FIGURE B-8
STIMULI EXPERIMENT 6: SINGLE BRAND PAIR

First Learning Phase	Second Learning Phase
Trial 1 (repeated 2 times)	*Trial 1 (repeated 2 times)*
Whitewater Raft nr. S541 *** BRAND: HYPALON** * MADE IN: SWEDEN * WARRANTY: 1 YEAR This raft is **HIGH QUALITY**	Whitewater Raft nr. S379 *** BRAND: HYPALON** * MADE IN: SWEDEN * WARRANTY: 1 YEAR *** FLOOR: TUBULAR** This raft is **HIGH QUALITY**
Trial 2 (repeated 2 times)	*Trial 2 (repeated 2 times)*
Whitewater Raft nr. S223 *** BRAND: RIKEN** * MADE IN: SWEDEN * WARRANTY: 1 YEAR This raft is **LOW QUALITY**	Whitewater Raft nr. S714 *** BRAND: RIKEN** * MADE IN: SWEDEN * WARRANTY: 1 YEAR *** FLOOR: I-BEAM** This raft is **LOW QUALITY**
Trial 3 (repeated 2 times)	*Trial 3 (repeated 2 times)*
Whitewater Raft nr. S316 *** BRAND: LEAFIELD** * MADE IN: SWEDEN * WARRANTY: 1 YEAR This raft is **HIGH QUALITY**	Whitewater Raft nr. S684 *** BRAND: LEAFIELD** * MADE IN: SWEDEN * WARRANTY: 1 YEAR *** FLOOR: TUBULAR** This raft is **HIGH QUALITY**
Trial 4 (repeated 2 times)	*Trial 4 (repeated 2 times)*
Whitewater Raft nr. S853 *** BRAND: BERING** * MADE IN: SWEDEN * WARRANTY: 1 YEAR This raft is **LOW QUALITY**	Whitewater Raft nr. S169 *** BRAND: BERING** * MADE IN: SWEDEN * WARRANTY: 1 YEAR *** FLOOR: I-BEAM** This raft is **LOW QUALITY**

Note: For both learning phases, trial types 5 to 8 were identical to types 1 to 4, except for the level of the Uncorrelated filler feature, which was Canada. Trial types 5 to 8 were each repeated 2 times in both phases.

FIGURE B-9
STIMULI EXPERIMENT 6: TWO BRAND PAIRS

First Learning Phase	Second Learning Phase
Trial 1 (repeated 3 times)	*Trial 1 (repeated 3 times)*
Whitewater Raft nr. S541 * **BRAND: HYPALON** * MADE IN: SWEDEN * WARRANTY: 1 YEAR * **FABRIC: POLYURETHANE** This raft is **HIGH QUALITY**	Whitewater Raft nr. S379 * **BRAND: HYPALON** * MADE IN: SWEDEN * WARRANTY: 1 YEAR * **FLOOR: TUBULAR** This raft is **HIGH QUALITY**
Trial 2 (repeated 3 times)	*Trial 2 (repeated 3 times)*
Whitewater Raft nr. S223 * **BRAND: RIKEN** * MADE IN: SWEDEN * WARRANTY: 1 YEAR * **FABRIC: POLYURETHANE** This raft is **LOW QUALITY**	Whitewater Raft nr. S714 * **BRAND: RIKEN** * MADE IN: SWEDEN * WARRANTY: 1 YEAR * **FLOOR: I-BEAM** This raft is **LOW QUALITY**
Trial 3 (repeated 3 times)	*Trial 3 (repeated 3 times)*
Whitewater Raft nr. S316 * **BRAND: HYPALON** * MADE IN: CANADA * WARRANTY: 1 YEAR * **FABRIC: NEOPRENE** This raft is **HIGH QUALITY**	Whitewater Raft nr. S684 * **BRAND: HYPALON** * MADE IN: CANADA * WARRANTY: 1 YEAR * **FLOOR: TUBULAR** This raft is **HIGH QUALITY**
Trial 4 (repeated 3 times)	*Trial 4 (repeated 3 times)*
Whitewater Raft nr. S853 * **BRAND: RIKEN** * MADE IN: CANADA * WARRANTY: 1 YEAR * **FABRIC: NEOPRENE** This raft is **LOW QUALITY**	Whitewater Raft nr. S169 * **BRAND: RIKEN** * MADE IN: CANADA * WARRANTY: 1 YEAR * **FLOOR: I-BEAM** This raft is **LOW QUALITY**

FIGURE B-10
STIMULI EXPERIMENT 7: UNCORRELATED EXTRA PHASE 1 ATTRIBUTE

First Learning Phase

Trial 1 (repeated 3 times)

Whitewater Raft nr. S541
* **BRAND: HYPALON**
* MADE IN: SWEDEN
* WARRANTY: 1 YEAR
* **FABRIC: POLYURETHANE**

This raft is **HIGH QUALITY**

Trial 2 (repeated 3 times)

Whitewater Raft nr. S223
* **BRAND: RIKEN**
* MADE IN: SWEDEN
* WARRANTY: 1 YEAR
* **FABRIC: NEOPRENE**

This raft is **LOW QUALITY**

Trial 3 (repeated 3 times)

Whitewater Raft nr. S316
* **BRAND: HYPALON**
* MADE IN: CANADA
* WARRANTY: 1 YEAR
* **FABRIC: POLYURETHANE**

This raft is **HIGH QUALITY**

Trial 4 (repeated 3 times)

Whitewater Raft nr. S853
* **BRAND: RIKEN**
* MADE IN: CANADA
* WARRANTY: 1 YEAR
* **FABRIC: NEOPRENE**

This raft is **LOW QUALITY**

Second Learning Phase

Trial 1 (repeated 3 times)

Whitewater Raft nr. S379
* **BRAND: HYPALON**
* MADE IN: SWEDEN
* WARRANTY: 1 YEAR
* **FLOOR: TUBULAR**

This raft is **HIGH QUALITY**

Trial 2 (repeated 3 times)

Whitewater Raft nr. S714
* **BRAND: RIKEN**
* MADE IN: SWEDEN
* WARRANTY: 1 YEAR
* **FLOOR: I-BEAM**

This raft is **LOW QUALITY**

Trial 3 (repeated 3 times)

Whitewater Raft nr. S684
* **BRAND: HYPALON**
* MADE IN: CANADA
* WARRANTY: 1 YEAR
* **FLOOR: TUBULAR**

This raft is **HIGH QUALITY**

Trial 4 (repeated 3 times)

Whitewater Raft nr. S169
* **BRAND: RIKEN**
* MADE IN: CANADA
* WARRANTY: 1 YEAR
* **FLOOR: I-BEAM**

This raft is **LOW QUALITY**

FIGURE B-11
STIMULI EXPERIMENT 7: PREDICTIVE EXTRA PHASE 1 ATTRIBUTE

APPENDIX C
EXAMPLES OF DEPENDENT MEASURES

Binary Quality Judgments

You will now be presented with information about 8 types of rafts. Based on what you've learned about each aspect in terms of whether it predicts quality in whitewater rafts, you have to indicate the quality level for each raft. To answer, circle the most appropriate answer.

Raft A
* BRAND: HYPALON
* MADE IN: CANADA
* WARRANTY: 1 YEAR
* FLOOR: TUBULAR

QUALITY: HIGH LOW

Raft B
* BRAND: RIKEN
* MADE IN: SWEDEN
* WARRANTY: 1 YEAR
* FLOOR: I-BEAM

QUALITY: HIGH LOW

Raft C
* BRAND: RIKEN
* MADE IN: SWEDEN
* WARRANTY: 1 YEAR
* FLOOR: TUBULAR

QUALITY: HIGH LOW

Raft D
* BRAND: HYPALON
* MADE IN: SWEDEN
* WARRANTY: 1 YEAR
* FLOOR: I-BEAM

QUALITY: HIGH LOW

Raft E
* BRAND: RIKEN
* MADE IN: CANADA
* WARRANTY: 1 YEAR
* FLOOR: TUBULAR

QUALITY: HIGH LOW

Raft F
* BRAND: HYPALON
* MADE IN: SWEDEN
* WARRANTY: 1 YEAR
* FLOOR: TUBULAR

QUALITY: HIGH LOW

Raft G

* BRAND: RIKEN
* MADE IN: CANADA
* WARRANTY: 1 YEAR
* FLOOR: I-BEAM

QUALITY: HIGH LOW

Raft H

* BRAND: HYPALON
* MADE IN: CANADA
* WARRANTY: 1 YEAR
* FLOOR: I-BEAM

QUALITY: HIGH LOW

<u>Instructions to Binary Quality Judgments Measure in Experiment 5</u>

You will now be presented with 8 additional raft profiles. Based on what you've learned about each aspect in terms of whether it predicts quality in whitewater rafts, you have to <u>indicate the quality level for each raft</u>. To answer, circle the most appropriate answer.

Before you start answering, it is important to give you some more information about whitewater rafts. Whitewater rafts are rugged floating devices used to navigate mountain rivers and streams. All rafts have air-filled flotation compartments and a floor. The fabric used for the compartments is usually polyurethane, but can also be neoprene. There are only two types of floors, I-beam floors and tubular floors. Some rafts also have a rudder and some have a frame for extra rigidity. Most rafts you can buy have a 1-year warranty. There are several manufacturers in the whitewater rafts market, including Hypalon, Riken, and Leafield. The countries that produce most rafts are Canada,

Sweden, the USA, and France. Thus, whitewater rafts have many different specs and features, so many, in fact, that most information sources can provide only part of the information. For example, two water sports catalogs may be selling exactly the same rafts, but one water sports catalog may tell you where the rafts are built, whereas another catalog may go into more detail about the technical specs of the exact same rafts. In fact, the 12 rafts you saw in the first set of profiles on the computer (before you took a break) were exactly the same rafts as the 12 rafts you saw in the second set of profiles on the computer (i.e., the rafts that came after you took a break).[1] Despite the fact that you will have incomplete information again about the next 8 rafts, we would like you to try to give the most appropriate answer.

Quality Judgments on Seven-Point Scale

You will now be presented with information about 8 types of rafts, some you've seen before, some you haven't seen before. Based on what you've learned about each aspect in terms of whether it predicts quality in whitewater rafts, you have to indicate the quality level for each raft. To answer, circle the most appropriate answer.

Raft A
* BRAND: RIKEN
* MADE IN: SWEDEN
* WARRANTY: 1 YEAR
* RUDDER: BACKBAR

-3	-2	-1	0	1	2	3
LOW QUALITY			AVERAGE QUALITY			HIGH QUALITY

[1] This sentence ("In fact, …break.") was omitted in the Control condition.

Raft B
* BRAND: HYPALON
* MADE IN: CANADA
* WARRANTY: 1 YEAR
* RUDDER: BACKBAR

-3	-2	-1	0	1	2	3
LOW QUALITY			AVERAGE QUALITY			HIGH QUALITY

Raft C
* BRAND: RIKEN
* MADE IN: CANADA
* WARRANTY: 1 YEAR
* RUDDER: FOURCASE

-3	-2	-1	0	1	2	3
LOW QUALITY			AVERAGE QUALITY			HIGH QUALITY

Raft D
* BRAND: HYPALON
* MADE IN: CANADA
* WARRANTY: 1 YEAR
* RUDDER: FOURCASE

-3	-2	-1	0	1	2	3
LOW QUALITY			AVERAGE QUALITY			HIGH QUALITY

Raft E
* BRAND: HYPALON
* MADE IN: SWEDEN
* WARRANTY: 1 YEAR
* RUDDER: BACKBAR

-3	-2	-1	0	1	2	3
LOW QUALITY			AVERAGE QUALITY			HIGH QUALITY

Raft F
* BRAND: RIKEN
* MADE IN: SWEDEN
* WARRANTY: 1 YEAR
* RUDDER: FOURCASE

-3	-2	-1	0	1	2	3
LOW QUALITY			AVERAGE QUALITY			HIGH QUALITY

Raft G
* BRAND: HYPALON
* MADE IN: SWEDEN
* WARRANTY: 1 YEAR
* RUDDER: FOURCASE

-3	-2	-1	0	1	2	3
LOW QUALITY			AVERAGE QUALITY			HIGH QUALITY

Raft H
* BRAND: RIKEN
* MADE IN: CANADA
* WARRANTY: 1 YEAR
* RUDDER: BACKBAR

-3	-2	-1	0	1	2	3
LOW QUALITY			AVERAGE QUALITY			HIGH QUALITY

WILLINGNESS-TO-PAY JUDGMENTS (EXPERIMENT 1)

Next you will see 9 whitewater rafts. Some of those rafts are types of rafts you have not seen before. What we would like you to do is to <u>guess the price for each raft</u> based on

what you've learned in the computer task. In other words, we would like you to write down, for each raft, the price that you think would be a reasonable price for that raft.

This may be a difficult task, but you know that the average price for whitewater rafts on the market (both stores and catalogs) is $2,000, with a price range of $1,500 for the cheapest rafts you can buy and $2,500 for the most expensive rafts. Think well about your answers. Note your answers on the dotted line below each profile. Before you start to write down the prices, look closely at all 9 rafts to get a good feel for the types of rafts you are asked to judge.

Raft #1
* BRAND: ENSOLITE
* COMPARTMENTS: AIRECELL
* HULL: POLYURETHANE
* FLOOR: TUBULAR

Price: $...................

Raft #2
* BRAND: RIKEN
* COMPARTMENTS: AIRECELL
* HULL: POLYURETHANE
* FLOOR: I-BEAM

Price: $...................

Raft #3
* BRAND: HYPALON
* COMPARTMENTS: AIRECELL
* HULL: POLYURETHANE
* FLOOR: BATTEN

Price: $...................

Raft #4
* BRAND: HYPALON
* COMPARTMENTS: AIRECELL
* HULL: POLYURETHANE
* FLOOR: I-BEAM

Price: $...................

Raft #5
* BRAND: ENSOLITE
* COMPARTMENTS: AIRECELL
* HULL: POLYURETHANE
* FLOOR: BATTEN

Price: $...................

Raft #6
* BRAND: ENSOLITE
* COMPARTMENTS: AIRECELL
* HULL: POLYURETHANE
* FLOOR: I-BEAM

Price: $...................

Raft #7
* BRAND: RIKEN
* COMPARTMENTS: AIRECELL
* HULL: POLYURETHANE
* FLOOR: TUBULAR

Price: $...................

Raft #8
* BRAND: HYPALON
* COMPARTMENTS: AIRECELL
* HULL: POLYURETHANE
* FLOOR: TUBULAR

Price: $...................

Raft #9
* BRAND: RIKEN
* COMPARTMENTS: AIRECELL
* HULL: POLYURETHANE
* FLOOR: BATTEN

Price: $...................

Willingness-To-Pay Judgments (Experiment 2)

Next you will see 3 sets of two whitewater rafts. Imagine again that you are shopping for a raft. Based on what you've learned about rafts in the computer task, you now have to decide how much you would be willing to pay for different types of raft. You do this by comparing rafts and determining how much more or less you would be willing to pay for one raft compared to other rafts. You know the price of the one raft, $2000, and you also know that the most expensive rafts on the market cost $2500 while the cheapest rafts cost $1500. So, for example, if the one raft costs $2000 and you would think another raft is worth $300 more than the one raft, then you would write "$300" and circle "MORE" below the profile of the second raft. If you would believe that the second raft is worth $200 less than the first raft, then you would write "$200" and circle "LESS" below the profile of the second raft.

Before you start to write down the prices, look closely at all 3 combinations of rafts to get a good feel for the types of rafts you are asked to judge.

Raft #1
* BRAND: RIKEN
* MADE IN: CANADA
* WARRANTY: 1 YEAR
* RUDDER: BACKBAR

Price: $2000

Raft #2
* BRAND: HYPALON
* MADE IN: CANADA
* WARRANTY: 1 YEAR
* RUDDER: BACKBAR

I would be willing to pay $............ MORE LESS for raft #2 than for raft #1.

Raft #1
* BRAND: RIKEN
* MADE IN: CANADA
* WARRANTY: 1 YEAR
* RUDDER: BACKBAR

Price: $2000

Raft #3
* BRAND: RIKEN
* MADE IN: CANADA
* WARRANTY: 1 YEAR
* RUDDER: FOURCASE

I would be willing to pay $............ MORE LESS for raft #3 than for raft #1.

Raft #1
* BRAND: RIKEN
* MADE IN: CANADA
* WARRANTY: 1 YEAR
* RUDDER: BACKBAR

Price: $2000

Raft #4
* BRAND: RIKEN
* MADE IN: SWEDEN
* WARRANTY: 1 YEAR
* RUDDER: BACKBAR

I would be willing to pay $............ MORE LESS for raft #4 than for raft #1.

Co-Occurrence Judgments

We have found that participants sometimes get confused when we show them a lot of information. We would like to ask you a few questions to see if this is the case.

Try to remember the rafts in the second set of rafts you saw on the computer (i.e., the set of rafts you saw after taking a break)?

In the second set of rafts, how many of the HIGH QUALITY rafts had a FOURCASE RUDDER? (circle the most appropriate answer)

0%	25%	50%	75%	100%
None, all high quality rafts had a backbar rudder	Most high quality rafts had a backbar rudder	Half of the high quality rafts had a fourcase rudder, half had a backbar rudder	Most high quality rafts had a fourcase rudder	All high quality rafts had a fourcase rudder

In the second set of rafts, how many of the LOW QUALITY rafts had a FOURCASE RUDDER? (circle the most appropriate answer)

0%	25%	50%	75%	100%
None, all high quality rafts had a backbar rudder	Most high quality rafts had a backbar rudder	Half of the high quality rafts had a fourcase rudder, half had a backbar rudder	Most high quality rafts had a fourcase rudder	All high quality rafts had a fourcase rudder

In the second set of rafts, how many of the rafts with FOURCASE RUDDER were made by HYPALON BRAND?

0%	25%	50%	75%	100%
None, all high quality rafts had a backbar rudder	Most high quality rafts had a backbar rudder	Half of the high quality rafts had a fourcase rudder, half had a backbar rudder	Most high quality rafts had a fourcase rudder	All high quality rafts had a fourcase rudder

In the <u>second</u> set of rafts, how many of the rafts with BACKBAR RUDDER were made by HYPALON BRAND?

0%	25%	50%	75%	100%
None, all <u>high</u> quality rafts had a backbar rudder	Most <u>high</u> quality rafts had a backbar rudder	Half of the <u>high</u> quality rafts had a fourcase rudder, half had a backbar rudder	Most <u>high</u> quality rafts had a fourcase rudder	All <u>high</u> quality rafts had a fourcase rudder

In the <u>second</u> set of rafts, how many of the HIGH QUALITY rafts were HYPALON BRAND? (circle the most appropriate answer)

0%	25%	50%	75%	100%
None, all <u>high</u> quality rafts had a backbar rudder	Most <u>high</u> quality rafts had a backbar rudder	Half of the <u>high</u> quality rafts had a fourcase rudder, half had a backbar rudder	Most <u>high</u> quality rafts had a fourcase rudder	All <u>high</u> quality rafts had a fourcase rudder

In the <u>second</u> set of rafts, how many of the LOW QUALITY rafts were HYPALON BRAND? (circle the most appropriate answer)

0%	25%	50%	75%	100%
None, all <u>high</u> quality rafts had a backbar rudder	Most <u>high</u> quality rafts had a backbar rudder	Half of the <u>high</u> quality rafts had a fourcase rudder, half had a backbar rudder	Most <u>high</u> quality rafts had a fourcase rudder	All <u>high</u> quality rafts had a fourcase rudder

In the second set of rafts, how many of the HIGH QUALITY rafts were MADE IN CANADA? (circle the most appropriate answer)

0%	25%	50%	75%	100%
None, all high quality rafts had a backbar rudder	Most high quality rafts had a backbar rudder	Half of the high quality rafts had a fourcase rudder, half had a backbar rudder	Most high quality rafts had a fourcase rudder	All high quality rafts had a fourcase rudder

In the second set of rafts, how many of the LOW QUALITY rafts were MADE IN CANADA? (circle the most appropriate answer)

0%	25%	50%	75%	100%
None, all high quality rafts had a backbar rudder	Most high quality rafts had a backbar rudder	Half of the high quality rafts had a fourcase rudder, half had a backbar rudder	Most high quality rafts had a fourcase rudder	All high quality rafts had a fourcase rudder

In the second set of rafts, how many of the HIGH QUALITY rafts had a 1 YEAR WARRANTY? (circle the most appropriate answer)

0%	25%	50%	75%	100%
None, all high quality rafts had a backbar rudder	Most high quality rafts had a backbar rudder	Half of the high quality rafts had a fourcase rudder, half had a backbar rudder	Most high quality rafts had a fourcase rudder	All high quality rafts had a fourcase rudder

In the second set of rafts, how many of the LOW QUALITY rafts had a 1 YEAR WARRANTY? (circle the most appropriate answer)

0%	25%	50%	75%	100%
None, all high quality rafts had a backbar rudder	Most high quality rafts had a backbar rudder	Half of the high quality rafts had a fourcase rudder, half had a backbar rudder	Most high quality rafts had a fourcase rudder	All high quality rafts had a fourcase rudder

<u>Predictiveness Ratings</u>

Now, we would like you to rate each of the following aspects independently as to whether it predicts the quality level of rafts:

The rating scale goes from *-10: this characteristic definitely <u>does not</u> predict the quality level* to *+10: this characteristic definitely predicts the quality level. 0* means *don't know.*

Please give your answers by circling the most appropriate value. For example, if you would think that brand doesn't matter at all in order to determine quality in rafts, you would circle -10 for BRAND. If you would think that brand matters a lot for raft quality, you would circle +10. Think well about your answers. Try to be as accurate as possible.

BRAND:

-10	-9	-8	-7	-6	-5	-4	-3	-2	-1	0	1	2	3	4	5	6	7	8	9	10
Definitely <u>does not</u> predict quality level										Don't know								Definitely predicts quality level		

COMPARTMENTS:

-10	-9	-8	-7	-6	-5	-4	-3	-2	-1	0	1	2	3	4	5	6	7	8	9	10

Definitely does not predict quality level — Don't know — Definitely predicts quality level

HULL:

-10	-9	-8	-7	-6	-5	-4	-3	-2	-1	0	1	2	3	4	5	6	7	8	9	10

Definitely does not predict quality level — Don't know — Definitely predicts quality level

FLOOR:

-10	-9	-8	-7	-6	-5	-4	-3	-2	-1	0	1	2	3	4	5	6	7	8	9	10

Definitely does not predict quality level — Don't know — Definitely predicts quality level

APPENDIX D

TABLES RAW WILLINGNESS-TO-PAY JUDGMENTS

TABLE 23
EXPERIMENT 1: MEAN FEATURE EFFECTS ON RAW WILLINGNESS-TO-PAY JUDGMENTS

	Mean Feature Effects ($)		
Type of 1st Predictive Feature / Feature	Control	Experimental	Difference
Brand			
Brand	243	422	178
Floor	545	147	-398
Attribute			
Attribute	338	675	337
Floor	437	217	220

TABLE 24
EXPERIMENT 1: RAW WILLINGNESS-TO-PAY JUDGMENTS AT NEW LEVEL OF FIRST PREDICTIVE FEATURE

	Mean Feature Effects ($)[a]		
Type of 1st Predictive Feature	Control	Experimental	Difference
Brand	570	175	395
Attribute	360	120	240

[a]Differences in prices between leafield rafts with a tubular floor versus an i-beam floor.

TABLE 25
EXPERIMENT 2: MEAN FEATURE EFFECTS ON RAW WILLINGNESS-TO-PAY JUDGMENTS

	Mean Feature Effects ($)								
	Control			Experimental			Difference		
Prior Beliefs / Feature	12	24	All	12	24	All	12	24	All
Low Prior Beliefs									
Brand (A)	145	183	165	307	297	302	162	114	137
Rudder (B)	173	192	183	204	178	189	31	-14	6
High Prior Beliefs									
Brand (A)	42	118	77	125	150	137	83	-32	60
Rudder (B)	304	345	323	-17	92	38	-321	-253	-285

BIOGRAPHICAL SKETCH

Stijn van Osselaer was born in Leuven, Belgium, on July 15, 1971. At two years of age, he moved to Bavel, the Netherlands. He attended the Gymnasium Bredanum grammar school (high school) in Breda, the Netherlands. At age 18, he returned to Leuven to attend the Catholic University of Leuven. At the Catholic University of Leuven, he obtained the degrees of Kandidaat in de Psychologie, graduating "with distinction," and the degree of Licentiaat in de Psychologie, for which he graduated "with great distinction." He wrote his thesis on intellectual styles and human decision processes under the guidance of Professor Paul De Boeck. As part of his studies for the Licentiaat degree, Stijn van Osselaer spent three months doing research with Professor Paul Webley at the Economic Psychology Unit of the University of Exeter in the United Kingdom and four months in Brussels, Belgium, as a trainee at the Organisation and Human Resources Management Unit of Coopers and Lybrand Management Consulting Services. Encouraged by Professor Piet Vanden Abeele, Stijn van Osselaer applied to several Ph.D. programs in marketing in the United States of America. He chose to attend the University of Florida, where he arrived in August of 1994. During his four years at the University of Florida, he was involved in several research projects co-authored by Professors Joseph Alba, Chris Janiszewski, and Barton Weitz at the University of Florida, Professors Luk Warlop at the Catholic University of Leuven, S. Ratneshwar at the University of Connecticut, and Sandy Jap at the Massachusetts Institute of Technology.

He participated in seminars taught by Professors Chris Janiszewski, S. Ratneshwar, Barton Weitz, Alan Sawyer, Joseph Alba, Steven Shugan, Gordon Bechtel, Joffre Swait, Joel Cohen, Barry Schlenker, and John Lynch. In 1996, he taught an undergraduate course in consumer behavior at the University of Florida.

I certify that I have read this study and that in my opinion it conforms to acceptable standards of scholarly presentation and is fully adequate, in scope and quality, as a dissertation for the degree of Doctor of Philosophy.

Joseph W. Alba, Chairman
Professor of Marketing

I certify that I have read this study and that in my opinion it conforms to acceptable standards of scholarly presentation and is fully adequate, in scope and quality, as a dissertation for the degree of Doctor of Philosophy.

Christopher A. Janiszewski
Associate Professor of Marketing

I certify that I have read this study and that in my opinion it conforms to acceptable standards of scholarly presentation and is fully adequate, in scope and quality, as a dissertation for the degree of Doctor of Philosophy.

Barton A. Weitz
J. C. Penney Eminent Scholar of Marketing

I certify that I have read this study and that in my opinion it conforms to acceptable standards of scholarly presentation and is fully adequate, in scope and quality, as a dissertation for the degree of Doctor of Philosophy.

Alan D. J. Cooke
Assistant Professor of Marketing

I certify that I have read this study and that in my opinion it conforms to acceptable standards of scholarly presentation and is fully adequate, in scope and quality, as a dissertation for the degree of Doctor of Philosophy.

Joffre D. Swait, Jr.
Assistant Professor of Marketing

I certify that I have read this study and that in my opinion it conforms to acceptable standards of scholarly presentation and is fully adequate, in scope and quality, as a dissertation for the degree of Doctor of Philosophy.

Joel F. Houston
Associate Professor of Finance,
Insurance, and Real Estate

This dissertation was submitted to the Graduate Faculty of the Department of Marketing in the College of Business Administration and to the Graduate School and was accepted as partial fulfillment of the requirements for the degree of Doctor of Philosophy.

August 1998

Dean, Graduate School